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..THE..
NEW YORK ACADEMY OF MEDICINE
17-19 WEST 43d STREET
NEW YORK

Dec. 14/02

Last vol. (pub. of the Trans. N.Y.
A. M. is Vol. 12. N. S. 1895.

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John S. Billings

TRANSACTIONS

OF

THE NEW YORK

Academy of Medicine.

INSTITUTED 1847.

SECOND SERIES. VOL. XII.

FOR 1895.

PRINTED FOR THE ACADEMY.
1897.

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Stated Meeting, January 3rd, 1895.

THE HISTORY OF THE TREATMENT OF SPONDYLITIS AND SCOLIOSIS BY PARTIAL SUSPENSION AND RETENTION BY MEANS OF PLASTER-OF-PARIS BANDAGES,

TOGETHER WITH

THE PRESENT STATUS OF THIS PLAN OF TREATMENT BEFORE THE PROFESSION OF THE WORLD.

By LEWIS A. SAYRE, M. D.

When any new mode of treatment is advanced in medicine or surgery, its advocates are apt to be overenthusiastic, and many of their readers, misled by apparent brilliancy of immediate results, often fall into extravagant praise, which further investigation leads them to modify; and, at times, discoveries which have been heralded as striking boons to humanity have afterwards been so completely forgotten that subsequent investigators have supposed them original when they rediscovered them for a second time, as was so admirably shown in the historical sketch of spinal supports read before you last year by Dr. Phelps.

It is now twenty years since I first applied plaster-of-Paris jackets for the treatment of Pott's disease and lateral curvature of the spine, and when your president did me the honor to ask me to read a paper before you, it seemed to me that I could find no better way of occupying your time than by presenting for your consideration a *résumé* of the origin of the plan, and of the opinions of the medical

profession of the world at large in regard to this mode of treatment after this lapse of time. That the treatment has not received universal acceptance is plain from a reading of the medical journals, though, in my opinion, the objections which have been advanced against it are, for the most part, due to a lack of comprehension, on the part of objectors, of the principles which I have advocated, and their failure to properly carry out the mode of applying these principles; while, on the other hand, some of the more enthusiastic advocates of this method have gone to the other extreme, and claimed for it more than I have ever asked. As twenty years have now elapsed since I first practised this method of treatment, it may not be without interest if I briefly review the manner in which I first came to adopt it.

For many years I was in the habit of treating most of my cases of Pott's disease with the "Taylor brace." I used to treat those who were too poor to pay for the instrument by applying what I called the "turtle shell" to the back.

This was made by laying the child on its belly across my lap, with the legs hanging over one side and the arms over the other, I then separating my legs so as to extend the spine and straighten it as much as possible, and until the child was perfectly easy and comfortable, which was made manifest in many cases by the improved respiration and the disappearance of grunting or jerking of the diaphragm.

While the child was held in this position a piece of linen or cotton cloth was accurately cut and shaped to the entire back, and two thirds around the entire trunk. In cases where the disease was in the cervical region the cloth was cut to extend to the top of the back of the head. Two or more pieces of cloth cut after the same pattern were then dipped in a mixture of plaster of Paris and water and laid smoothly over the first one. In the cases where the dis-

ease was in the cervical vertebræ, several thicknesses of this plaster bandage were put on the back of the neck, to give firm support to the head. Another piece of the cloth cut on the same pattern was then placed over the whole, and, while the spine was well stretched by separating my legs, an assistant applied a smooth roller bandage around the entire body and head, fitting it nicely into all the irregularities, and especially over the crest of the ilium, thus giving perfect support and preventing any motion of the spine.

When the plaster had become set, the child was then laid in the "turtle shell" on his back on a pillow, and the mother could sew a neat-fitting front to the sides of the plaster shell, and, lacing it in the centre from pubes to sternum, make it give firm support to the spine, while at the same time it could be removed for purposes of cleanliness.

In very many cases this simple plan of giving rest to the spinal column was attended with very satisfactory results, especially where the disease was in the cervical region, in which location, if the head and neck had been well extended while the plaster bandages were applied, and retained in this position till the plaster was set, the weight of the head was removed from the neck and supported by the shoulders by the closely fitting plaster-cast, and at the same time the whole spine was absolutely immovable—that is, no movements could take place between the separate bones.

In November, 1874, a little boy was brought to me having a sharp posterior curvature of the last three dorsal and the first lumbar vertebræ, and there was also partial paralysis of the rectum and one leg. As it was impossible to send the child to the hospital, as he did not live in the city, and I had no time to apply the "turtle shell," I felt compelled to devise some plan by means of which the boy could be made comfortable while being ransported to his home at Chatham Four Corners, Put-

nam county, New York, nearly one hundred and fifty miles distant. Having studied the subject for some time, and questioned myself regarding the propriety of completely incasing the trunk with a plaster dressing, I had finally resolved to make the experiment as soon as a suitable opportunity was offered. It seemed to me that the opportunity had then come, and that the circumstances justified the measure. Accordingly, I directed one of my assistants to suspend the boy by the arms in order to see what effect would be produced, and I noticed that, as soon as the boy was made pendent, there was more motion in the paralyzed limb than before, that the girdling pain around the belly was very much relieved, and that the patient was breathing much easier. While he was suspended in this manner, I pulled down his shirt and tied it between his legs, thus making it fit the body closely and smoothly, and then took some plaster-of-Paris bandages which had been prepared in the ordinary manner to be used in the treatment of diseased ankle joints, and, commencing at the pelvis between the trochanters and the crest of the ilium, completely encircled the entire trunk to the axilla. At first I was anxious concerning the effect that would be produced upon the respiration, but, inasmuch as the boy cried lustily, all my fears in that direction were quickly dispelled, and I went on, reversed the bandage, brought it back to the pelvis, and so went on until four or five thicknesses of the roller were made to completely embrace the body. He was kept suspended by the hands of my assistant for about twenty minutes, until the plaster of Paris became hard. He was then laid upon his face on a sofa, and was to remain there until the plaster had become firmly set. I left him upon the sofa and went to my lunch, and when I returned, I found, to my complete astonishment, that the little fellow had got up and walked across the office, and was then looking out of the window. Still fearing that respiration might be interfered with too much, I cut through the dressing from the top of the sternum to the pubes, thus allowing it to gap considerably and permit of a more complete expansion of the chest. *The boy, however, did not feel as comfortable after as before the incision through the front part of the jacket was made.* I then applied a roller bandage around the pelvic portion of the jacket and again brought its edges together,

but left the upper portion to separate as much as the movements of respiration seemed to require. In order to give security to the upper, or thoracic, portion, and at the same time to permit free expansion of the chest walls, I took the mother's elastic garter, cut it into six strips, fastened them upon each side of the slit in the jacket, and then tied them in front.

The dressing now being completed, I requested the parents to return with the child at the end of a week or ten days, when I would have a Taylor's brace ready and would adjust it. The plaster jacket had been put on simply for the purpose of rendering the child comfortable while being carried home. That was the last I saw of either the child or the parents until the following February, four months and a half, when he again returned.

In the meantime the boy had grown considerably, looked healthy, was able to walk without assistance, and was not obliged to support the upper portion of the trunk by placing his hands upon his knees. Without waiting to make any further examination, I at once took the patient in my carriage and started for my clinic at Bellevue Hospital Medical College. The streets were covered with holes and elevations formed by the ice and snow, and the jolting was almost intolerable, yet the boy made no complaint whatever. That fact of itself showed how efficient the dressing was for securing absolute rest to the diseased parts. At the college the jacket was removed in the presence of the class, when it was found that the curvature was much less, but now the boy was unable even to sit up. As soon, however, as the dressing was reapplied, the mother remarked: "He can now sit up and walk again."

This, in brief, is the history of the first case in which I applied the plaster dressing completely around the body from the pelvis to the axilla. Since that time it has constituted almost the only treatment which I have adopted for Pott's disease.

The proper manner of applying plaster-of-Paris bandages in spondylitis.—Cross-barred muslin or crinoline should be the material used in making the bandages. Frequently the kind of sizing used in the manufacture of

this muslin prevents the plaster from setting quickly. It should therefore be washed to get rid of the superfluous sizing before being torn into strips from three to four inches in width and three yards in length. Of course the selvage is to be torn off. These strips are drawn through a tray filled with freshly ground plaster of Paris, and enough rubbed into the muslin to fill all the meshes. The bandages are then rolled moderately tight and laid in an air-tight tin until required for use.

The patient should have the body covered with a tightly fitting knitted or woven woolen shirt, without sleeves, tied tightly over the shoulders and drawn down and securely pinned over a folded towel in the perinæum. For this purpose a safety pin should always be used. If the patient is a female, pads of proper thickness should be placed over the mammæ and under the shirt, which pads are to be removed when the plaster sets, thus preventing pressure on the glands. Another towel, folded in such a shape as to cover the stomach and bowels, called the "dinner pad," which also is to be removed after the plaster sets, is placed inside the shirt, thus providing space for the expansion of the stomach during the process of digestion. If the patient has partaken of a hearty meal just previous to the application of the plaster, this dinner pad may be omitted.

The patient being now prepared for the application of the plaster, is placed in the suspending apparatus, which has been heretofore accurately described in my several works on this subject, care being taken that the traction be evenly divided between the head and axillæ.

Traction is now made very slowly and gently, and only carried to the point of giving the patient perfect comfort, and never beyond that point.

In some cases the heels will be slightly raised from the floor before this point is reached, but in many instances the heels will not be raised from the floor at all; and as the sensations of the patient are the only guide as to the

amount of traction needed, an anæsthetic should under no circumstances ever be given, as has been done by some surgeons, as one requires the intelligent coöperation of the patient himself in regard to the amount of traction required. If it is a child not old enough to talk and tell its feelings, watch carefully the expression of its countenance; and when it is changed from pain to pleasure, there stop, never making traction beyond that point, and immediately apply the plaster bandages with great care and accuracy, pressing them into all the irregularities and covering the entire trunk from the pelvis to the top of the sternum.

If the patient is kept suspended in this position till the plaster is set, it will retain the body in the position of perfect comfort which suspension has given it.

In applying the bandages, one should be placed on end in a basin or pail of tepid water, *deep enough to completely submerge it*, when bubbles of gas will at once begin to escape. As soon as the bubbles cease, the plaster will be all moistened and the bandage ready for use. Do not add salt to the water, as it renders the plaster brittle. Squeeze out the superfluous water before applying it, and place another roll, end up, in the water, which will be ready for use by the time the first one has been applied, and proceed thus until the entire jacket has been completed. The reason for placing the bandages on end in the water is that the gas may escape and thus enable all the plaster in the roll to be evenly moistened. If laid on the side, the moisture will only extend to some parts, leaving others dry and unfit for use.

The patient being suspended, the jacket is applied by the surgeon, standing or sitting at the back of the patient, while an assistant sits in front, steadying the patient by his knees and rubbing and smoothing the bandages which are being applied.

Begin at the waist, taking one or two turns around the smallest part of the body, and then going down in a spiral

form, each layer overlapping the other half or two thirds of the width of the bandage until reaching the trochanters; then, having taken one or two turns around the pelvis, reverse the bandage, and gradually proceed in the same spiral manner upward until you have covered the entire body to the top of the sternum.

This process is repeated till there are a sufficient number of thicknesses to support the body, the number of bandages to accomplish this object, of course, depending on the size of the patient.

In cases where the disease is in the lower dorsal or lumbar vertebræ this is all that is required. If the disease is at the mid-dorsal or cervical vertebræ, it then becomes necessary to add the jury-mast to the jacket in order to take off the weight of the head from the vertebral column.

In many instances great advantage is derived from the addition of Whitman's shoulder brace to keep the chest well expanded, and press the shoulders back into the jacket.

In the great majority of cases the jacket can be applied while the patient is suspended vertically, with the greatest ease to himself and the surgeon; but in exceptional cases where there is paralysis, where the heart is too weak to allow the patient to remain upright for any length of time, or in cases of excessively fat and feeble people, Davy's hammock, with holes cut to allow projection of the head and feet, is to be preferred, traction being made at both ends of the body to the point of comfort while the jacket is being applied.

Now, to pass to the subject of lateral curvature of the spine, a deformity which has nothing in common with spondylitis, except that they both affect the spine—and I might say in passing that, surprising as it may seem, there appears to be in the mind of the profession at large very often a hazy mingling of these two troubles, which bear about the same relation to each other as tumor albus and

genu valgum, and, although I have written repeatedly and at length on the different modes of employing plaster of Paris in the treatment of these two affections, it seems to me, from the papers I read in the various medical journals, that a number of physicians are still under the erroneous impression that I use gymnastic exercises in cases of Pott's disease, and employ immovable plaster jackets in cases of lateral curvature. In the treatment of lateral curvature the position that I hold is, that a large number of cases if seen early may best be treated by improvement of the general tone of the system, increasing muscular strength, and educating the muscular sense of the patient to the correct carriage of the body without the aid of any mechanical support whatever. In other cases it is not sufficient to rely upon the patient alone, but recourse must be had to artificial support to hold the trunk in a correct position until the patient is capable of voluntarily maintaining it so; and in a third class, where marked deformity is present, oftentimes accompanied by great pain and disability, artificial aid is often necessary to support the body in an improved position during the entire life. One of the most useful means, in my experience, of rectifying lateral curvature of the spine has been vertical, partial self-suspension accompanied by manipulation, and with me nothing has been so serviceable in holding this improved position as a corset made of flexible material applied to the body in its improved position, which, then hardening, retains this improved shape; and of the various plastic materials that I have tried, plaster of Paris has answered my purpose better than anything else. I wish to raise my voice in emphatic protest against what many gentlemen seem to regard as my method of treatment in lateral curvature—namely, placing the patient in a plaster jacket and leaving her there for a number of months, then removing the jacket and replacing it by a second. The plaster corset is simply for the purpose of holding such improvement in

position as you have been able to obtain by your manipulations, and does not in any way improve the shape of the patient beyond this, and should be renewed as often as improvement in the form justifies, and worn only so long as the patient is unable voluntarily to maintain an equally good position without its aid; and during the treatment it is essential that the corset be removed while the patient exercises daily for the purpose of gaining that increased physical health and muscular tone which are necessary to the successful treatment of lateral curvature of the spine.

In applying plaster-of-Paris bandages in cases of lateral curvature, a shirt of double length is used, pads are placed over the mammæ, and a strip of tin two inches wide is placed next the skin from the sternum to the pubes, on which to cut off the plaster; and, instead of being suspended by the head and axillæ, the patient suspends herself by pulling on the free end of the rope which passes from the head-swing over the pulley, while she keeps the arms outstretched, the upper hand being on the concave side. As soon as the plaster is set, which should be the case by the time the corset is finished, it is split open down the front and removed while the patient is still suspended. A thin slice is then taken from each edge of the slit and the corset held together by a roller bandage and dried. When dry, the next day, it is put on the patient while again self-suspended, and fastened by a roller bandage; then trimmed out under the arms and above the thighs until comfortable, and removed. The extra length of shirt is then reversed over the jacket and sewed to itself, covering in all the plaster, and lacings are sewed on in front.

Without wearying you further by speaking of my own ideas on these two subjects, I would pass to the opinions of others, as any treatment which will not stand the test of time and criticism is not worthy of acceptance.

While the first demonstrations of this method of treating Pott's disease and lateral curvature were received with

marked enthusiasm in all parts of the world, wherever the demonstrations were made, it remained to be seen whether, after the lapse of twenty years, the same enthusiastic approval will be accorded to it, and I accordingly have written the following letter to some hundred of the most distinguished orthopædic surgeons in various parts of the world:

285 FIFTH AVENUE, NEW YORK, Nov. 19, 1894.

DEAR SIR:—I have been invited to read a paper before the New York Academy of Medicine on January 3, 1895, on The History of the Treatment of Spondylitis and Scoliosis by Partial Suspension and Retention by Plaster-of-Paris Bandages—setting forth the present status of this form of treatment before the medical profession of the world after twenty years of experience.

Will you kindly let me have, at your earliest convenience, your views on this subject, whether for or against the plan of treatment, together with such other suggestions as you may choose to make, in order that I may embody them in my report?

Trusting that I may have the pleasure of an early reply, I am, Yours very truly,

LEWIS A. SAYRE, M. D.

I have received answers from a large number of them, and have embodied extracts from them in this report. Those that are opposed to it I will read entire; of those in favor I will use only extracts:

Robert Jones, Liverpool, said: "I am not favorable to suspension in the treatment of lateral curvature, and would trust entirely to exercises, muscle-beating, and, where necessary, elevation of the heel of the boot."

E. Muirhead Little, London: "*In re* gypsum jackets, I have not used one for many years. I prefer poroplastic, felt, or steel supports."

Bernard Roth, London: "I have treated with much success upward of two thousand cases of scoliosis, including many of extreme deformity, by my method of posture and exercise, without any mechanical support whatsoever, and I never found suspension of any use in scoliosis.

"In reference to spinal caries, for the first ten years of my practice as an orthopædic surgeon I applied your plaster-of-Paris bandages almost exclusively with varying success, but for the last ten years I have discarded them altogether in favor of the late H. O. Thomas's spinal splint with axillo-spinal straps. I have modified this splint, so that the patient's back can be uncovered for examination, and the instrument replaced and refixed while the patient is lying prone the whole time."

Noble Smith, London: "Probably the most fallacious form of treatment is that of applying plaster-of-Paris jackets. Among the many enthusiastic surgeons who at first made use of them, there are very few having had much experience who have not now given them up as a failure. Plaster-of-Paris jackets ought to be utterly condemned, because, first, of their great weight; second, their interference with free respiration and cleanliness; and third, the fact that they act simply as heavy, unscientific corsets, pressing upon the chest, and add very little to the support of the spine."

B. E. McKenzie, Toronto: "With the exact manner of treatment referred to I have had but little experience. For several years I have employed some form of gymnastic treatment associated with the employment of forceful correction through suspension, with lateral traction, manipulation of the spine, etc. The objection which presents itself to me in the use of any retentive appliance is that it tends to atrophy and to produce weakness of the trunk instead of aiding in the development and strengthening of the parts."

H. Hodgen, of St. Louis, does not think he has ever seen a very good result in scoliosis from the use of the jacket, and prefers the steel assistant.

John Ridlon, Chicago: "I have used the permanent plaster-of-Paris jacket in spondylitis from time to time during the last twelve years, applied as you directed, with the patient partially suspended, when I have been unable for any reason to obtain an antero-posterior steel spine brace of the Taylor pattern. I have also used the plaster jacket in certain cases of spondylitis where the lateral deformity was so great that the steel brace could not be satisfactorily adjusted. In all those cases where the jacket

has been used for the posterior curvature simply I have not been able to prevent an increase of the deformity, and in all those cases that have come under my observation after having been treated by other surgeons, I have been given a history of steadily progressive increase of the deformity. . . . In those cases, however, where there has been marked lateral deformity I have found the deformity progressively diminish under treatment by the plaster jacket; but in these cases I have not continued the treatment with the jacket after the lateral deformity had disappeared and with the posterior curvature increased to any considerable extent.

"In scoliosis it has been my custom for the past two or three years to use the laced plaster jacket in all cases except the very slight ones. I use it in connection with partial suspension and pressure by the squeezing machine, and I am satisfied that I have obtained a much greater degree of correction of the deformity than I had been accustomed to obtain either with exercises alone, after Roth's method, or with the use of the leverage lateral brace formerly applied, and perhaps still used, by Dr. Shaffer of your city. Under treatment by Dr. Shaffer's brace I used to find nearly all my cases grow somewhat more deformed. None of them made any material gain, although treated for long periods. Under the exercise treatment of Roth very slight cases at times fully recovered; moderately severe cases gained somewhat, and rarely ever grew worse; in severe cases the deformity did not in any way diminish, and some few did not appear to receive any benefit whatever. At this time I am satisfied that the plaster corset used in connection with the squeezing machine and exercises gives in my hands the best result in the treatment of scoliosis."

James K. Young, Philadelphia: "In answer to your favor of November 28th, asking for my views upon 'The Treatment of Spondylitis and Scoliosis by Partial Suspension and Retention by Plaster-of-Paris Bandages,' I can best answer by quoting from my recent work, *Treatise on Orthopædic Surgery*, 1894, under the treatment of these two subjects:

"*Spondylitis.*—After describing the technique now employed, I say: 'While possessing certain advantages

of economy, requiring less special experience in its application, and being entirely beyond the control of the patient or attendants, there are certain positive disadvantages—the encircling of the body within a solid support, the inability to inspect the condition of the skin and note the progress of the affection, the formation of excoriations, ulcerations, and abscesses without the knowledge of the surgeon, the lack of cleanliness, etc.—which relegate it and its modifications to a secondary position. When split, furnished with lacings, and applied and removed at will, it loses part of its efficiency, but there is a gain in comfort and cleanliness. It is of decided value, however, for patients who are unable to bear the expense of even the cheapest apparatus; in such cases, with attention to detail, a cure may often be effected, and the writer has in public practice frequently proved this statement, especially when the disease was located in the lumbar region.

"In justice to this mode of treatment it must be added that there are certain cases in which it is the best and most efficient; this applies particularly to the lateral deviation of the spinal column present in certain cases of Pott's disease and in lower lumbar disease. It is, moreover, important that the surgeon be familiar with both the plaster-of-Paris jacket and the spine brace, adapting each to the special requirements of the individual case.' Individual experience leads me to employ the steel spine brace wherever it can be carefully adjusted at short intervals under personal supervision.

"*Scoliosis.*—I believe in and employ partial suspension, but do not now employ plaster-of-Paris jackets in the treatment of scoliosis. I prefer to treat these cases early by mild, long-continued movements, exercises, posture, and massage—methods by which I have achieved the greatest success."

Thomas G. Morton, Philadelphia: "In private practice I do not use the plaster apparatus you refer to, but at the Orthopædic Hospital we now and then resort to it when the patient is unable to procure the Lentz spinal apparatus which we commonly use."

Joel E. Goldthwait, Boston: "I would say that for routine treatment I use a form of back brace, practically the Taylor, reserving the suspension and jackets (plaster)

for those cases which the brace fails to hold, or in which there is lateral deformity.

"In these cases, if ambulatory treatment is desired, I think the jacket is by all odds the best form of treatment."

Charles L. Scudder, Boston: "I have nothing novel or new to add to the common experience of surgeons with this form of retentive apparatus. I would say simply that in cases of caries of the spine below the tenth dorsal vertebra in which for some reason or other it may be undesirable to use steel braces, I find the plaster-of-Paris jacket, applied well over the hips and up to the axilla, efficient.

"In the application of this plaster dressing I am inclined to use more and more the hammock as a means of extension rather than the tripod and sling."

Robert W. Lovett, Boston: "I have used in the treatment of lateral curvature the plaster jacket, I think in all its forms, and with certain modifications I still continue to use it. Believing as I do very firmly in gymnastic treatment as an essential part of the treatment, I have abandoned the use of the fixed jacket except in unusual cases, where I apply it to a patient who is forcibly corrected, leaving it on only for a short time. This measure I have found of use in some of the very severe cases. It is applied under suspension.

"A method which I have used extensively has been to apply a jacket to the patient while suspended, cut the jacket off down the front, and fill it with plaster, thus obtaining a cast of the body; then, by the method described by Dr. Bartow, of Buffalo, I remove the prominence on the convex side of the cast, building it up on the opposite side; in some cases over-correcting the deformity, in other cases bringing them nearly to the straight line. To this cast I apply a jacket over an under vest, cutting it down the front and having it provided with lacings. This I have been accustomed to use in connection with gymnastics and forcible correction. It represents the patient's condition as it would be if a decided improvement had taken place, of course. I am a believer in the efficiency of this 'corrected' jacket.

"I have also used braces to accomplish the same purpose. I can not account myself an advocate of the jacket to the exclusion of the brace, and I use sometimes one and

sometimes the other. In milder cases I depend for the most part on gymnastic treatment. I trust I have answered your inquiries."

133 NEWBURY STREET, BOSTON, Dec. 10, 1894.

MY DEAR DR. SAYRE:—In reply to your communication, let me say that I find the treatment of Pott's disease by partial suspension and retention by plaster-of-Paris bandages in a large number of cases of caries of the spine to be most beneficial. It is particularly adaptable for cases of disease below the seventh dorsal. In the convalescent stage I use removable jackets—either plaster, leather, paper, or aluminum removable corsets—where these are within the means of the patient. In a number of cases, however, I make use of braces. These are largely in the cases that are in the upper portion of the spine and where careful nursing can be given. In such cases, where thorough nursing can be given, I am in the habit of making an antero-posterior support. In certain acute cases a fixed plaster jacket seems to give a more thorough support than the brace as ordinarily applied. It has seemed to me that an antero-posterior support could be made to hold as well as a plaster jacket even in the portion of the spine to which the plaster jackets are most suitable, but practically, in the majority of cases of the acute sort, certainly in poorer practice, a plaster jacket will be found the readiest and most efficient. In lateral curvature I am also using plaster-of-Paris jackets with partial suspension, but I have combined with it also a later traction and direct pressure. Here, again, I am in the habit of using substitutes for plaster jackets—namely, leather, paper, and aluminum,

With sincere regards,

E. H. BRADFORD.

Henry O. Marcy, Boston: "I thank you for calling my attention to the paper which you have in preparation. I am sure that I can add very little of value upon the subject, since from the first I have considered you my master, and take pleasure in writing you the reasons which caused me to first seek your acquaintance. The first article which you published upon the treatment of angular curvature of the spine by means of suspension and the application of the plaster jacket impressed me so favorably that after its

careful study I went to New York for the sole purpose of seeking your instruction, and, thanks to your courtesy, I did not return until I had familiarized myself with every detail.

"It is very probable that I was the more favorably impressed with your method, since for some years I had used plaster bandages for almost every variety of fracture. . . . Another case which I cited, of a woman confined to the bed because of a dorsal curve, at once commenced her work after the application of the jacket, at which time she was three months pregnant. She refused to part with her jacket or have it replaced by another, chipping away in front as the enlarging abdomen demanded, until her labor commenced. No other jacket was applied, and somewhat recently I had the good fortune of examining her, all these years the resulting cure having remained complete with only a slight deformity. As you are well aware, procedures emanating from any other source have been accepted by the Boston profession only after repeated proofs of real merit. To this conservatism your teaching proved no exception. I was not alone the first to introduce plastic splints to the profession of Boston, but also your methods of the application of the plaster jacket, and unweariedly wrote and talked upon the subject. Much excellent work has been done here for a good many years, especially at the Children's Hospital, but unfortunately the general profession are still wanting in technique and training. I inclose you a reprint of 1877 upon 'Plastic Splints in Surgery,' and call your attention to its closing sentences. What I then said I now repeat, but more than all as the outcome of your teaching upon this subject do I prize a personal friendship."

ALBANY, N. Y., Dec. 14, 1894.

Lewis A. Sayre, M. D., New York City:

MY DEAR DOCTOR:—In reply to your letter of November 19th, I am reminded largely of the early history of your work in the treatment of spondylitis and scoliosis when first presenting the subject to the attention of the New York State Medical Society. You will remember that I was the first who presented a case—that of Miss M.—for your treatment, application of the bandages, etc., and the result was

quite decidedly satisfactory. While for the past five years my line of practice has been away from orthopædic surgery, yet my impressions, in answer to your questions, would be that my results in the treatment of angular curvatures of the spine have been decidedly satisfactory. In the treatment of lateral curvature I can not speak so emphatically. For angular curvature I applied the first plaster-of-Paris dressing put on in this city, after having read a report of one of your clinical lectures at Bellevue Hospital on the subject. I applied the bandages directly to the surface of the body, and had the pleasure of seeing the little patient do remarkably well.

Hoping these few stray notes will be of service to you, and that you will pardon my delay in answering, believe me,

Sincerely yours,

A. VANDER VEER.

Lewis Balch, Albany: "I have not applied a plaster jacket for six or eight years. From the time you put on one before the State Medical Society, in the winter of 1878—I think that was the date—up to 1886, I used the jacket for all cases of spinal curvature coming under my charge. In general I can say the treatment was successful, but I noticed in some cases of rotary lateral curvature the patient seemed to slip down through the jacket, no matter how carefully it was applied, and the treatment failed. Eventually I had better success in antero-posterior curvature than in the other.

"I know the jackets were properly applied, for you taught me how to put them on, both here and in your own office."

M. H. Burton, Troy: "I have used the Sayre jacket in treating cases of spondylitis and scoliosis in the Troy Hospital and in private practice with decided and manifest success. I have witnessed its use in Marshall's Infirmary, Troy, N. Y., and the results were not gratifying. I have seen cases also which had been treated by yourself, and found complete recovery the result. It would not be within my province to suggest to you in regard to the Sayre jacket or the counterpart appliances.

"Your experience and knowledge in the matter preclude such suggestions, if I had any to make—which I

have not. As compared with other jackets or appliances, I find no single point in their favor, so far as experience and observation give me knowledge.

"In all respects I find the Sayre plaster-of-Paris appliances or jackets the most efficacious, convenient, and valuable, and the ones causing the greatest success in the treatment of the diseases for which we apply them."

R. B. Bontecou, Troy: "Your letter of November 19th was duly received, and I have nothing more to state than what the profession at large have stated and will state in admiration of your device to accomplish physiological rest by fixation, which I believe is the great principle to observe in the treatment of disease of the spinal column and the greater joints. I have relieved and cured a multitude of suffering humanity by your method, both in hospital and private practice—adults as well as children. Of late years I have substituted water glass of heavy specific gravity for the plaster, finding it equally resistant and durable, and lighter, and can be cut open when necessary and used as a corset more readily than plaster.

"I have also, in children with posterior spinal deformity, often substituted the prone position in a hammock of cotton flannel, with advantage, utilizing the hammock as one of the envelopes. It is certainly more comfortable for children than the vertical position."

Ansel G. Cook, Hartford: "About eighty per cent. of my cases of spondylitis wear solid plaster-of-Paris jackets, with or without the jury-mast; about fifteen per cent., plaster or leather corsets made to lace in the front; and about five per cent., for various reasons (position of abscesses, imperfect development of hips, complications of other joints, etc.), wear various forms of braces. This does not include the very severe cases that are confined on wire or plaster and board cuirasses, or a class of cured or convalescent adults whose backs are not strong and who require some support. These do best, I find, with a light back brace, padded the whole length of the brace, worn under an ordinary pair of corsets.

"The mild cases of scoliosis I treat with exercises alone; the more severe, with exercises supplemented with braces that do not hold, but compel the patient to hold himself, in a corrected position (you will note the distinction); and

the most severe, with plaster-of-Paris or leather corsets. I hope your paper will touch on the subject of suspension, forcible correction and retention, which has so many enthusiastic advocates at present.

"A great deal of trouble, I think, arises from improperly made and badly fitting jackets; it certainly requires considerable practice to make a good one, and when a man condemns plaster jackets you are inclined to agree with him when you see the thing he has made and calls a jacket. On the other hand, the man who sends his patient to an instrument-maker to have a brace fitted, without giving accurate directions as to how the brace shall be made, and attending personally to the fitting of it, is not likely to get any satisfactory result."

BUFFALO, Dec. 5, 1894.

Dr. Lewis A. Sayre:

MY DEAR DOCTOR: In reply to your favor of November 19th, asking my views in the matter of the treatment of spondylitis and scoliosis by plaster-of-Paris bandages, I would say about as follows: First of all, I would wish to acknowledge the benefits which I, like all other surgeons, have received from the views upon treatment which you have published so widely and with which your name is so inseparably connected.

So far as the treatment of acute spondylitis is concerned, and at that time when absolute fixation is so absolutely necessary, I must say that I know of nothing that can take the place of a plaster-of-Paris corset. With regard to the later treatment at a time when plaster has become irksome, or when the acute stage is past and the patient's personal comfort can be more consulted, I must say that, for my own part, I think that other mechanical devices can be substituted which permit of sufficiently frequent removal to keep the patient more comfortable. I do think that braces or jackets of various kinds can be devised that give sufficiently complete fixation to meet the indications, and that are much more agreeable for the patients. But, in the early and very acute stages, and in a certain class of hospital and dispensary patients, I think that plaster or its equivalent, yet to be discovered, more completely fills the indications than anything else.

With regard to scoliosis, it is not my own personal cus-

tom to resort much to plaster jackets, except as I may put one on in order to obtain a cast of the body around which to construct a suitable brace. I think these cases are more often injured by too long rest of the muscles, and for all except the inveterate cases I much prefer a movable corset which will permit massage or very much more active muscle training. The more I study and have to do with lateral curvature, especially in the young and growing, the more I believe in the expediency of gymnastic treatment, and the less absolute reliance I like to place upon support. I may sum it all up by saying that in my estimation a happy combination of the two constitutes the best treatment.

I appreciate your request for my own views, which I advance with no little hesitation to such an authority as yourself, and am, very sincerely yours,

ROSWELL PARK.

KANSAS CITY, MO., Dec. 4, 1894.

Dr. Lewis A. Sayre, 285 Fifth Avenue, New York:

MY DEAR DOCTOR:—Yours of the 19th just to hand, and I would say in reply that in spondylitis of the upper dorsal and cervical region I have used with good results, and advocate recumbency. Where the disease is manifest below this part, I use suspension and retention by plaster-of-Paris bandages—fixed dressing if the trouble is active, and movable apparatus or laced plaster-of-Paris dressing if angular curvature has taken place. In scoliosis I find the suspension and retention by the plaster-of-Paris bandages, cutting down and lacing, my best method.

Your friend,

J. D. GRIFFITH.

MINNEAPOLIS, Dec. 3, 1894.

DEAR DR. SAYRE: In reply to your kind letter, would say in the majority of cases of spondylitis I use partial suspension and retention by plaster of Paris, and when I do use a steel support it is usually to cater to the prejudice of my patient, for since I have known how to apply the jacket properly I have been able to do more good to a greater number with it than with any other means at my command.

For the past two years in treating scoliosis I have aban-

doned braces and supports of every kind, and I believe with advantage to my patients, except in cases in which the deformity is increasing very fast, and then I apply a plaster jacket until I have had time to build my patient up and get her able to stand alone as it were.

Fraternally,

J. E. MOORE.

Hunter McGuire, Richmond, Va.: "Your letter of November 19th has been received. In the treatment of spondylitis nothing, in my opinion, is better than partial suspension and retention by plaster-of-Paris bandages. Having a son with this disease, there is nothing in medical literature on this subject that I have not studied. I have practised several other devices, but have never found anything equal to the plaster-of-Paris bandages. When I use them in scoliosis, however, I always arrange them so they can be removed; and endeavor to strengthen the muscles by massage, electricity, and gymnastics."

116 SOUTH EIGHTEENTH STREET,
PHILADELPHA, Nov. 28, 1894.

MY DEAR DR. SAYRE:—In all cases of spondylitis in the early stages, where the patient is not confined to bed, I use plaster-of-Paris bandages, as suggested by you. Later, in private practice, I use some form of steel supporting apparatus. In my hospital work and among the poor I usually employ plaster-of-Paris jackets from the beginning to the end of the treatment of the case, and at all times when the cost must be considered.

In scoliosis I use the plaster-of-Paris jacket, which is reapplied at short intervals, to support the spine and overcome the deformity, believing that we can accomplish better results in this way than by more expensive and cumbersome apparatus.

When the curvature has been overcome I apply some form of light supporting apparatus. Of course in scoliosis I do not neglect to use stretching and pressure, with gymnastic exercises, to increase the flexibility of the spine.

I shall be glad to give you more details if this short account is not sufficient. Believe me, very truly yours,

WILLIAM J. TAYLOR, M. D.

Nov. 23, 1894.

Dr. Lewis A. Sayre, 285 Fifth Avenue, New York:

DEAR DR. SAYRE:—In reply to your note of the 19th I would most gladly say that the profession owes much to you for developing the use of plaster of Paris as a surgical dressing in a great variety of cases and conditions. In regard to its use in spondylitis, it has been a Godsend to hundreds of poor people who would otherwise have been without this most economical and effective method of fixing the spinal column until repair takes place. I know of no method so convenient for producing absolute fixation of the bones of the spinal column. It certainly has its disadvantages; but your work in this line has stimulated efforts at substitution which have obviated its disagreeable features. The great principle underlying the treatment, however, that of rest and fixation, has steadily and persistently taken firm hold upon the profession at large. While method and means may change, the principle will live.

In regard to self-suspension for scoliosis, it is one of the helpful means of rectification, and I value it highly. I am opposed to apparatus for scoliosis, both on theoretical and practical grounds, whenever it can be avoided. I believe in strengthening the muscular power by active and passive gymnastic movements, as I consider them the essential elements in effecting a cure. At the same time I do not deny that there are cases rapidly growing worse which require permanent support.

When plaster of Paris is used in these cases, it should, in my judgment, be employed as a corrective agency, with constantly increasing pressure upon the projecting portions of the body, as advocated by yourself and by the plan of Bartow. Thanking you again for the earnestness with which you have pursued this branch of surgical work, I am,

Yours very sincerely,

DE FOREST WILLARD.

John Chalmers De Costa, Philadelphia: "The custom in the Philadelphia institutions with which I am connected is to treat acute and early spondylitis by means of rest in bed, and to rely entirely upon plaster of Paris, applied

during suspension by your method, for the completion of all cases and the cure of patients."

1611 SPRUCE STREET,
PHILADELPHIA, Nov. 30, 1894.

MY DEAR DR. SAYRE:—I am in receipt of your letter of November 19. I am very strongly in favor of the plan of treatment by plaster of Paris, applied during partial suspension, in cases of spondylitis below the mid-dorsal region, also as a means of holding a jury-mast in cases above mid-dorsal region. I am strongly opposed to the use of plaster of Paris applied in any position for scoliosis where it is allowed to remain for more than one week's time, because it tends to increase the muscular atrophy from disuse. I believe that it would interfere with the object of your paper did I do more than briefly state my views as above. I can but believe that the mechanical advantages of plaster of Paris have been seriously interfered with by the injudicious employment of it in unsuitable cases and in an unmechanical manner. It has often appeared to me that plaster of Paris was applied apparently with the idea that it acted as a therapeutic measure by absorption in some such way as many of the nostrums are supposed to act. It is only in this way that I have been able to explain the irrational manner of its application. I remember saying in one of my clinical lectures that it seemed a great misfortune that Professor L. A. Sayre could not communicate his skill in selecting the cases suitable for the method as well as his skill in applying plaster of Paris, to prevent the terrible abuse to which it has been subjected.

Yours most sincerely,
H. AUGUSTUS WILSON.

Edmund Andrews, Chicago: "The plan is a valuable one in many varieties of cases. One of the important advantages in the West is that family physicians in distant mountain and prairie regions, far away from instrument-makers, can always make use of it.

"Another advantage is that by its use the remote physician protects his patient from getting useless and injurious braces made by city instrument houses from inadequate measurements sent by mail.

"The practical difficulty as to its use by distant country practitioners is the tendency of some of them to leave it on without change so long that the skin becomes foul and offensive, and their neglect to guard the prominences of bone, thus producing ulcers by the pressure.

"As the patient may be ten or fifteen miles distant over bad roads, they think they can not go to him very often.

"It is a serious question for a far-off country doctor as to what he can do for a distant spinal case having scanty pecuniary means. My extensive correspondence with these people has impressed me strongly with their difficulties. I use the plaster jacket frequently, but in many other cases I prefer apparatus of other material."

B. Merrill Ricketts, Cincinnati: "In reply to your request for my opinion concerning the treatment of spondylitis and scoliosis by partial suspension and retention by plaster-of-Paris bandages, I will say that this method is, I believe, the best thus far presented when applied with skilful hands.

"*Its advantages.*	*Disadvantages.*
1. Non-elasticity.	1. Uncleanliness.
2. Perfect adjustment.	2. Excessive weight.
3. Inexpensiveness.	3. Liability to fracture.
4. Convenience.	4. Irritation by small particles of plaster.
5. Rapidity.	
6. Frequent reapplication.	5. Absorption of secretions.
7. Continuous adjustment.	

"To overcome the disadvantages, I have, as a rule, used the leather jacket supported by steel made from the original plaster cast. The leather jacket does not meet as many requirements as the plaster, but, until some of the disadvantages of the plaster are overcome, I believe it will be necessary to use the leather, believing that it excels the other substances, such as metal, wood, paper in any form, or any of the various apparatuses."

W. R. Whitehead, Denver: "I recently received your letter about spondylitis, and hasten to reply at my earliest convenience. I wish that I could add something really valuable and interesting to what I have already expressed concerning your treatment of this disease. I am sure, my dear doctor, that no one appreciates your efforts in this

matter better than those who have profited, as I have, by your instruction in the application of this the most valuable means of treatment, in my opinion, of Pott's disease of the spine, which is one of the most troublesome surgical affectons with which we have to contend.

"I am opposed to all stiff braces as inefficient, painful to the patient, and often a most useless expense. The trouble is, generally, that the Sayre jacket is not properly applied; it is almost always, in my experience, too heavy, or the plaster is of poor quality; sometimes the piano-felt pieces about the hips are omitted, or there is something about the jacket that shows inefficiency in its application, or a want of proper knowledge of, or proper appreciation of, the purposes for which it is applied, as simple as they are. These things are not the fault of the Sayre jacket, but the fault of those who sometimes attempt to avail themselves of this excellent device—indeed, the fault of the *medicus imperatus* with which this country is abundantly supplied. Take, for example, an intelligent country doctor, far removed from all the conveniences of a city, and if he understands how to apply your jacket properly, and has any ingenuity at all and a little good plaster of Paris, he can, in my humble opinion, make for his patient a better and more comfortable and more useful appliance for the treatment of spondylitis than all the nickel-plated steel braces and other spinal 'contraptions' offered for sale and advocated by instrument-makers and recommended by their inventors."

"P. S.—Some doctors, it seems, are invited to send east a mold of the bust in plaster, and a jacket will be furnished shaped after such a mold. My comments are that a doctor who has not experience and sense enough to make a suitable jacket can not make a good mold."

Henry M. Sherman, San Francisco: "I am very glad indeed to submit to you, my old master in my art, my experiences and conclusions in regard to the plaster-of-Paris jacket, and to let such of them as merit it reach the profession through your paper.

"I advance the following propositions:

"1. In cases of vertebral tuberculosis, when the lesion is below the ninth dorsal vertebra, the treatment by 'partial suspension and retention by plaster-of-Paris bandages'

is incomparably the best, always excepting those cases in which for special and obvious reasons—hernia, abscess, sinuses, etc.—no plan which contemplates the rigid enveloping of the body can be allowed. I am sure this contention can be made, though I have been obliged to modify my earlier views of the 'work' of a jacket both as a splint and as a support.

"2. In cases of vertebral tuberculosis, when the lesion involves the eighth and ninth dorsal vertebræ, the choice lies between the jacket and a brace, the former being suitable for milder cases, and those in which a compensatory lordosis could be easily developed both above and below the kyphos.

"3. In cases of vertebral tuberculosis, in which the lesion is above the eighth dorsal vertebra, the plan of treatment should include carriage of the head by apparatus, and in all such cases the supporting point of the mechanism is the pelvis, which is embraced by a jacket of plaster of Paris, or a pelvic belt of leather. For the support and carriage of the head I prefer the steel fork and aluminum chin collar which I have described.

"4. In cases of scoliosis the plaster-of-Paris jacket as a rigid, all-embracing splint on the trunk is the most satisfactory means to hold and make permanent the correction gained by gymnastics, forcible correction of deformity, etc.; but its function is purely that of a splint—*i. e.*, retention, not correction—and, like all splints, muscular weakness of the muscles corrected and aided by its results from its too exclusive and too prolonged use. I am myself working with a fair amount of success on the lines of these propositions. Of course, I cannot claim them to be universally applicable, and have myself exceptions to them in my practice, but they are a more than good set of working rules."

R. H. Plummer, San Francisco: "My experience in this method of treatment reaches through a period of fifteen years, during which time I have applied it not only in private practice, but in Cooper Medical College Dispensary as well, and it affords me great pleasure at this time to testify to you in no uncertain terms my appreciation of the merits of the method of treatment. . . . That the method has not given unqualified satisfaction to all who

have attempted to use it goes without saying. Failures arise from various causes, as improperly selected cases (incurable); improper materials used, as unsuitable shirts; unskillful application of the bandages; and a general injudicious management of the case as to exercise, etc. All these deficiencies may be summed up in four words—viz., 'ignorance of the method.' If all who essay to use it would first familiarize themselves with your writings upon the subject, and especially if they could avail themselves of the benefit of your personal instruction as to details, etc., the method would not suffer at their hands, and their patients would invoke blessings on their lives."

L. C. Lane, San Francisco: "I fully approve of your method of suspension and retention by plaster-of-Paris bandages in the treatment of spondylitis and scoliosis. I regard this plan of treatment as one of the most important additions made to surgery during the present century."

S. H. Pinkerton, Salt Lake City: "In the treatment of spondylitis I consider the partial suspension and a properly applied plaster-of-Paris jacket (with its modifications) superior to all mechanical appliances.

"In scoliosis I have had very good results with a removable plaster-of-Paris jacket (or corset) together with gymnastics."

William E. Wirt, Cleveland: "It has been my experience as an orthopædic surgeon to have applied at least two hundred, possibly double that number, of plaster-of-Paris jackets to cases of Pott's disease. I consider this jacket a great boon to these unfortunate children, especially when the disease occurs among the poor and ignorant—in fact nothing can take its place. Among the wealthy and intelligent, and more especially those whom we may trust to carry out directions, I have found a modified Beely corset to be more satisfactory. In certain cases of Pott's disease, where I had ordered a spinal brace, I have found that no amount of fitting and adjusting would make it comfortable to the patient, and in such cases would have to return to plaster jacket or felt corset.

"In scoliosis I do not favor the plaster jacket as a measure of treatment, but I simply use it as a means of getting a mold which I still further improve in shape (over that gained by suspension) before molding it upon a felt corset."

S. A. Wright, Manchester: "In reply to your question:

"1. I have never used any fixed apparatus for scoliosis.

"2. For caries of the mid and lower dorsal and lumbar region I have always used, and do still use, Sayre's jacket largely, but not in cases where there is an abscess, nor in cases where the disease includes the upper dorsal region. I am not strongly impressed with the value of any form of jury-mast, and where the jacket is inapplicable I prefer bed, or a metal spine spring."

C. H. Golding-Bird, London: "*Re Scoliosis: (a) Plaster Bandages.*—After a continued trial of the bandages in these cases I abandoned them; not that they did not at first give the greatest support, keep the spine straighter, allow of the chest being kept (in bad cases) more expanded, but because I found these results to be but temporary. In spite of the exercise with the tripod I did not find that the musculature of the trunk improved to the degree requisite for the patient's maintenance, as the improved position is to be obtained rather by the best possible development of the muscles (whereby the wearing and wearying muscle pain attendant on these cases is abolished and the patient rendered able to be about with comfort) and not by the most perfectly shaped back, if the muscle tone can not at the same time be maintained to the full. I less and less, therefore, use supports, preferring to throw the onus of maintaining an improved position on the patient's own muscles, being careful to give alternately with calisthenics such rest as in each case is needful.

"Where patients are already cripples and a lifelong support is necessary, then doubtless the jacket gives the maximum of comfort; but in practice I have found, in every station of life, that where such support is requisite the patient after a time prefers an apparatus (felt or otherwise) that can be removed for toilet purposes to the plaster that is immovable and that has to be reapplied at stated intervals, even at a loss of some of the support that the bandage gives.

"*Re Scoliosis: (b) The Tripod Exercise.*—This can hardly be improved upon; and it remains the type of what technical calisthenics in these cases should be.

"With due attention to the patient's environment of place and habit, the muscles soon develop and harden under its proper use; and it brings the greatest number of trunk muscles into play at a given time, with a minimum of fatigue; in this surpassing other and more popular forms of exercise.

"Even in confirmed scoliotic cripples, I have seen the daily expansion of the chest by the tripod exercise improve the heart's action and materially aid in the restoration of the patient's health, by mechanically permitting the better oxygenation of the blood and the freer action of the heart.

"I consider it of more value than the plaster jacket in the Sayre method of treating scoliosis.

"*Re Spondylitis: (a) The Plaster Jacket.*—In spondylitis the jacket fulfills all that its inventor claimed for it, allowing patients to be about and in the open air instead of being kept always on their backs; and they recover, where the jacket is efficiently and sufficiently often applied, with as slight deformity as though they had been kept lying down all the time.

"I know of no disability attending the employment of it important enough to prevent its universal application; but at the same time, I have never known any treatment introduced into surgery that was less understood, or about which less trouble was taken to understand it practically.

"While I still employ it in public practice, it is many years since I did so in private. The agitations of those who had 'vested interests' in spinal cases, and the prejudice of the public against an appliance that could not be removed for the bath, in a few years caused it to fall into disrepute, and save for hospital cases I do not think it is ever now employed.

"The public refused to regard the benefit the jacket afforded as in any way weighing against any slight disagreeables it might possess; and an otherwise intelligent mother informed me once that she declined to go on with a treatment in which a surgeon's personal attention was necessary at intervals of two or three months, when her instrument-maker could supply her with a steel and leather support that would last for years! And this may be taken as a fair sample of what many said or implied.

"When I add that leading instrument-makers advertised

in the past that they put on 'Sayre's jackets' for the profession, and also that leading surgeons sent their cases to them for the purpose, and when I know of my own knowledge that the jackets were nothing but stiff plaster belly-bands, rapidly excoriating the flanks, and worse than useless, and their uselessness made known as demonstrative of the futility of 'Sayrism'—it is not surprising to find that the plaster jacket had to give way to steel, leather, and felt, which materials now reign supreme!

"However, the lesson taught by Sayrism has not been lost entirely; felt jackets, though an imperfect support, more nearly fulfill the principles of that system than the old steel and leather supports; and even those who still use the latter endeavor to get them more body-fitting than formerly.

"*Re Spondylitis: (b) The Tripod Exercise.*—I only use the tripod for the application of the jacket. In a stage when exercise might indeed somewhat unfold the spine by extension upon recently formed reparative material, I should consider its use as an exercise unwise; while where consolidation has occurred, it would be useless.

"The exception I make is in the case of deformity, after consolidation, being such in the dorsal region as to cause the chest to assume the position of deep expiration; then exercise, by acting through the thoraco-humeral muscles, undoubtedly, as in old scoliotics, gives for the time free heart-play and better mechanical opportunity for the oxygenation of the blood, and thus the general condition of the patient is much improved."

Edmund Owen, London: "For a good many years I used suspension in the treatment of children with Pott's disease when applying plaster-of-Paris jackets; but, in due course, experience showed me that I got equally good results by applying the jackets as the child stood on the ground, the arms being held up out of the way. I am not a believer in extension of a carious spine. I found the plaster-of-Paris jacket treatment cheap, handy, and efficient; but, as a kind Providence has removed me from further charge of out-patients, I no longer have occasion to apply the jackets personally. Still, I have firm faith in them, and constantly get them applied by my house surgeons and dressers in Pott's disease.

"I consider their use in lateral curvature (curvature with rotation) to be injurious in childhood; though I freely admit that in the case of some limp and crumpled adults with lateral curvature, a gypsum corset may be applied with great advantage, especially if the patient be suspended during the application. Indeed, without suspension the jacket would be of little avail.

MacNaughton-Jones, London: "In reply to your letter I send you the photograph of the first patient on whom a plaster-of-Paris jacket was put in Ireland in the year 1876. It was adjusted by me prior to your visit to Cork that same year. Since that date to the present I have practised the treatment there advocated by you, so far as suspension and support are concerned. Necessarily of late years my work has not carried me so much into this field of surgery as it did in Ireland. As you know, I published the results of my cases up to a certain date in the Dublin *Monthly Journal of Medical Science*. You want to know what is my present view of the results of your plan of treatment in spondylitis and scoliosis.

"*First, Spondylitis.*—I regard the plaster jacket well adjusted in the early stages of the disease as the most valuable support to the diseased structures I know of (I include in this statement all the hybrid deviations which imitators of your method have adopted), so long as the integrity of the support is secured and maintained. This, so far as I can judge, is essential. The head support I also include in this view. The treatment in the early stages should be, I hold, combined with a fair amount of rest in the horizontal position and all the other hygienic and locomotive aids which are so essential in this treatment.

"Given a fair case and prudent supervision with judicious change of the support, it is the treatment which I should adopt in the case of a child of mine affected with the disease. Certain cases of spondylitis, in which general tubercular affections of the various bones are present, can be cured by no treatment I know of. Deformity is inevitable, do what we will. I do not suspend in Pott's disease save in placing the jacket on the patient. This latter is no hindrance to treating the complications of the disease. This I have proved many times. In the latter stages,

when the support has done its work, I resort to the well-fitting poroplastic jacket with steel stays and the softened portion for the affected area. This I can combine with light arm supports and the head rest if necessary. This is my view of the plan you advocated, and which has stood (though so often twisted and turned for some faddist's personal advertisement as hardly to be recognized as yours) the test of the last twenty years. If the principle has failed, it has been from trial when no cure was possible, in cases in which nothing was to be expected from any plan of treatment, or it has been carried out ineffectually and imperfectly.

"*Scoliosis*.—I cannot speak too warmly of my experience of the suspension treatment in scoliosis. I may have departed in a degree from my original practice in always employing the plaster jacket, perchance unwisely in some instances. I now rarely, in the cases that I am consulted in, use plaster. I use the poroplastic felt, carefully molded and applied under suspension, in all cases. I support the jacket with stays of steel where required, and combine at times light axilla props. I invariably insist on the practice of suspension by the neck at home, and combine the treatment with gymnastic exercises and the usual rules of hygiene and diet followed in such cases. I can unhesitatingly say that such treatment gives but the best results. I have never, since I put on the jacket I send you the photograph of, resorted to any other mode of treatment in any form of lateral curvature. I believe it to be the best. As a general rule, it has given me such good results that I have never been tempted to go outside it. Even in old and irremediable cases it has given the greatest comfort and relief from pain.

"In conclusion, let me, as one who has on many an occasion reaped the benefit of your inventive skill and sound practical teaching in this branch of orthopædic surgery, as in many others, pay no mere flattering tribute to the system of treatment you so graphically introduced into this country. But the real testimony to its worth is one you may neither see nor hear. It is the unrecorded and unspoken gratitude of the many whose lives have been spared, whose sufferings have been mitigated, and deformities removed through your own work, and not of those who have followed in your footsteps. This is true

of the past, it is equally so of the present, and it is my belief it will be so in the future."

E. Luke Freer, Birmingham: "My experience with plaster-of-Paris jackets dates from Dr. Sayre's demonstration in Birmingham some fifteen years ago, one of the patients being supplied by me from the Birmingham Royal Orthopædic Hospital, where I then held the post of honorary assistant surgeon. In that capacity I applied every plaster jacket for my senior colleagues (in those days everything 'spinal' was turtle-shelled indiscriminately), and I might almost have been dubbed 'honorary plasterer' to the hospital. As might have been expected, the results were not universally satisfactory. As honorary surgeon, I have dealt only with my own cases, but my applications have numbered over three thousand altogether, and I am now as firmly convinced as ever of the superiority of the plaster cuirass over all other methods for the treatment of spondylitis; where recumbency was necessary in cases of abcess, etc., I have found Thomas's double hip splint very serviceable. In high dorsal and cervical caries I have preferred Furneaux Jordan's modification, and in some cases Thomas's collar, to the jury-mast, as giving more complete fixation. In scoliosis (unassociated with spondylitis) I still use the cuirass, but only when the patient has to pursue his or her ordinary vocation, or at the commencement of treatment, until the muscles and muscular sense have been sufficiently re-educated and strengthened by systematic and harmonious kinetics and the correction of faulty positions, never when reclining or performing the exercises. I consider the constant use of the cuirass prejudicial in these cases, and I always use the divided cuirass with elastic lacing in scoliosis. I am convinced that the opposition to the cuirass has been mainly due to improper application, and the omission of the long 'reflected' vest which meets all objections on the score of 'chipping.' Elaborate instruments with ratchets and pads I consider are worse than useless—the tortures of the Spanish Inquisition would seem to be the models on which some of them were fashioned.

"If I may be allowed to speak freely, I think that much of the present antagonism to the cuirass is due to the fact that Professor Sayre, in his excellent and explicit monograph, was rather too sanguine in stating that any practi-

tioner could apply it. Many of the so-called plaster jackets I have seen were simply burlesques of the principles involved, and in this country the application has most frequently been relegated to nurses. I know of no operation, however, that depends more on strict attention to minor details for success, which details are only to be learned by experience. Poroplastic I have entirely discarded, as it is impossible to get it perfectly and accurately molded to the body, even where the deviation from the normal is slight. Again, it has nothing to recommend it even on the scores of expense or weight. Several of my cases have worn the divided plaster cuirass for more than twelve months with perfect comfort, the weight being no more than poroplastic. I continue to use partial suspension also in addition to other kinetics, especially in high dorsal curves and torticollis, with the best results."

W. Bayard, St. John, N. B.: "It affords me much pleasure to say to you, and I do so in all sincerity, that I consider your 'plaster jacket' a boon to the profession and to the unfortunate sufferer with spondylitis. I have used it largely for many years—indeed, I use none other—and I can point with pride to individuals walking our streets who, but for Sayre's jacket, would in all probability be in their graves. I have added various materials to the plaster with the view of rendering it less brittle, but I have found its porosity injured, so that I now use nothing but the plaster.

"In some instances I have inserted strips of steel, such as are used in ladies' corsets, between the bandages with benefit. When applying the bandage I always allow the patient's feet to touch the floor, and if he complains of pain in his legs I cease suspending, and I open the jacket for ablution purposes.

"I prefer the plaster jacket for scoliosis to any mechanical application I have seen, but I never trust to it alone, recommending such muscular exercises as the particular case demands.

"I may mention a traumatic case where the jacket answered admirably. A man was sitting under a heavy wheel when the supports broke; the wheel caught him on the shoulders, doubling him forward. I saw him immediately after the accident; he had no power in his legs, but yelled with the pain in them. I had him placed face

down, with a man at each shoulder and at each foot, directed to make steady extension, while I made gentle pressure upon the prominence produced by the altered position of the vertebræ. In a moment or two the power of motion returned and the pain ceased. He was taken home on a door; the next day I placed your jacket on him, and kept him in bed ten weeks. He wore a jacket for two years, after which he was as strong as ever."

J. Nicolaysen, Christiania, Norway: "I have used your treatment of spondylitis and scoliosis by partial suspension and retention by plaster-of-Paris bandages since the time you first brought it forward for the public.

"I have always been very content with the results of this treatment; the results have naturally been more or less perfect after the condition of the patients, especially after the stadium of the disease, when the treatment could be instituted; but always the condition has been bettered, in most cases for the life; I know not of any better treatment."

Enrique M. Porto, Havana: " . . . In conclusion: In angular curvatures of the spine the Paris-plaster bandage is the only and exclusively local treatment, and in light lateral curvature partial suspension and exercise combined is a good treatment, but I find it better if aided by retention of body by plaster-of-Paris bandage."

CORK, IRELAND, Nov. 30, 1894.

Since 1877 I have treated most of my cases of spondylitis by means of the plaster-of-Paris jacket, using very slight extension, always taking care not to break up or injure any bony adhesion of the spinal column.

The great majority of patients treated in this manner did well. When this treatment failed, rest on a couch, together with some form of extension, was adopted, and after a time the plaster-of-Paris jacket was reapplied.

With regard to scoliosis, I have had the great privilege of seeing many cases treated by Professor Sayre himself. I have adopted, and highly approve of, his method of treatment by extension, using the exercises recommended by Dr. Reginald Sayre, and the cuirass as a support during the day in severe cases. But I have substituted the felt poroplastic jacket for the plaster cuirass, because I found

that, owing to the extreme dampness of our climate, I was not as successful with my plaster-of Paris work as Dr. Sayre had been with his.

NICHOLAS GRATTAN.

H. Hingston, Montreal, Canada: "In those cases of curvature arising from disease when uniform and comfort-giving support is of advantage, no surgical appliance, however well fashioned it may have been, gives me anything like the satisfaction I have had in the use of the simple and inexpensive support Dr. Lewis A. Sayre has given to the profession."

PITTSBURGH, PA., Dec. 31, 1894.

Dr. Lewis A. Sayre, New York:

DEAR DOCTOR:—I find that I will be unable to have the paper now in preparation, upon Spondylitis and Scoliosis, completed in time to send you an advance copy, as per promise. It will not likely, however, contain anything that would be of particular value to you in the preparation of your paper. My treatment for the cases is based upon the plan as carried out by yourself and others. I use suspension, gymnastics, forcible correction, and retention by plaster-of-Paris bandages.

Shall look forward with a great deal of interest for the appearance of your paper, which I know will be of great value to us. Yours truly,

S. L. MCCURDY.

Dr. Redard, Paris: "In answer to the questions which you have been kind enough to ask me, I would say, I am a convinced partisan, first, of suspension and plaster jackets in the treatment of scoliosis; second, of plaster jackets in the treatment of Pott's disease at a certain stage.

"In the treatment of scoliosis I recommend vertical suspension in cases of slight rigidity of the spinal column, and daily exercises as a preparation for the application of plaster jackets.

"Vertical suspension is the basis of a great number of exercises carried out with the different apparatus which are described in my work on Orthopædic Surgery.

"In a great number of cases where there is pronounced rigidity of the spinal column, I use my apparatus for lateral suspension.

"The removable corset gives excellent results in a great number of cases of scoliosis which, in my opinion, however, are not obtained solely by its use, but in conjunction with proper exercises to strengthen the vertical muscles and restore the flexibility and proper position of the spinal column.

"It is of particular value where the spine is movable and not rigid, and where suspension corrects the deformity to a great extent; in these cases the faulty attitude is partially overcome, the lateral curve is very much modified, and the height of the patient is increased. In the interval between the exercises the patient is well supported, the pressure on the various points of the thorax forces him to take a correct position, and he does not lose the benefit gained by the various exercises used to correct the deformity.

"To sum up, the plaster corset is of particular value as a means of support after the rigid spinal column has been rendered flexible by the methodical use of exercises properly carried out.

"Experience has taught me that it exercises very little influence on the deformity of the ribs, or the twisting of the spinal column.

"I would further say I do not use plaster corsets in the early stages of scoliosis. I recommend it in cases where there is a lateral flexion and where the vertical curve has been partially overcome by the use of vertical suspension. I have obtained excellent results in some cases of scoliosis in its second stage, by combining the treatment of plaster jackets and exercises with mechanical force in straightening the spinal column. The plaster jacket has been particularly valuable in my hospital practice, in very marked cases of scoliosis accompanied by pain and disturbances both of circulation and respiration. In these cases it has been of great benefit by supporting the body, preventing injurious compression of the organs of the thorax and abdomen, and greatly diminishing the pain.

"In the treatment of Pott's disease, when the disease is in the cervical region or accompanied by paraplegia, I employ suspension, placing the patient in an oblique or vertical position on a padded board.

"In the first stage of Pott's disease I prefer absolute rest in the horizontal position. I have obtained excellent

results from the application of the plaster jacket at the period of reparation and consolidation of the spinal column, when it has been possible to prevent deformity and often pain has entirely disappeared, the result of entire rest in the horizontal position.

"According to my experience in some exceptional cases, the plaster jacket prevents, to some extent, the deformity in Pott's disease.

"I do not agree with some authors that this jacket can give extension to the two segments of the spinal column, and reduce pronounced curvature. The principal function of the apparatus is to obtain absolute rest and immobility for the diseased parts, and thus arrest the progress of the disease.

"In cases of Pott's disease in the cervical region the plaster jacket is of little use."

From this number of letters which, I take it, expresses pretty fairly the general opinion of the medical profession, I find six who condemn the plan altogether, or, at any rate, say that they do not use it; seven who say that in certain cases it is of use, and in others less servicable in their hands than different means; and thirty-five who employ this mode of treatment altogether. Among the objections to their use, which have been set forth in the letters I have received, are those of Noble Smith, who says that "plaster-of-Paris jackets ought to be utterly condemned—first, because of their great weight; second, their interference with free respiration and cleanliness; and, third, the fact that they act simply as heavy, unscientific corsets, pressing upon the chest, and add very little to support the spine." I may simply answer these objections by saying that a properly made plaster jacket is but slightly heavier than a steel support, and in many instances is even lighter; that, when properly applied, embarrassment of respiration is caused by the pain which free respiratory movements give the spine in Pott's disease, and not on account of constriction of the plaster jacket; and that, as far as cleanliness goes, the patients who are clean before

the application of the jacket, and are properly attended to by careful parents or nurses, remain clean, and that the accumulated evidence of many prominent physicians is a better refutation than anything I personally can say of the charge that they are "heavy, unscientific corsets, pressing upon the chest, and adding very little to support the spine."

Of the various substitutes that have been used instead of plaster of Paris, silicate of sodium can be as accurately fitted; but it is so much longer in setting that it is extremely difficult to make a good-fitting jacket on the patient, unless a plaster shell is placed outside of it to retain it in position until the silicate of sodium has become hard, or else the silicate jacket is made over a model, which adds greatly to the trouble of making the jacket. It also has the disadvantage of not allowing the insensible perspiration to pass through it, as does the plaster of Paris. This is apparent from the illustration of these two models, which represent two plaster jackets, covered in, top and bottom, with a small hole in one end to allow of the insertion of the stem of a tobacco pipe. One has been varnished and the other has not. Through the unvarnished one you see the smoke passes freely when I blow it inside the jacket; the other is impervious. In regard to felt, paper, wood, aluminum, leather, and celluloid, I would say that the felt, to my mind, is decidedly inferior to the plaster of Paris, being very little lighter, and in hot weather much more offensive. The objection to leather is that it is more liable to warp, and also the fact that it becomes offensive in hot weather. The celluloid, wood, paper, and aluminum jackets are lighter than the plaster of Paris, but have this disadvantage, that they require the services of a skilled artisan to construct them, and that they are beyond the reach of people of small means, who live at a distance from large towns, and a practical experience with them, as worn by different patients and manufactured by different surgeons,

has shown me that they are much more difficult to adjust, so as to give comfort to the patient equal to that afforded by the plaster jacket. The principle, however, is that for which I strive—namely, the adapting of some plastic material to the body in case of Pott's disease, in a position of comfort, and in lateral curvature in the position of best possible form, for the purpose of retaining the body continually in these improved positions—and the material of which the jacket is constructed is not essential. so long as the principle is carried out, though I think, in the vast majority of cases, that plaster of Paris will be found greatly superior to the other materials that I have mentioned on account of its cheapness and ease of application.

APPENDIX.

BERLIN, Dec. 23, 1894.

DEAR DR. SAYRE:—Owing to an unaccountable error I was under the impression that you would not deliver your address before January 15th, and therefore I had hoped to employ some leisure time at Christmas in fulfilling your request. To my great dismay, I see that the date is January 3d, and not 15th, and that it is scarcely possible now for my answer to reach you in time.

I should feel inconsolable for this mistake were it not for the conviction that I can give you no information of importance, so that for you it is a matter of comparative indifference whether you receive my answer before or after your address.

From your great good nature I will trust that you will pardon me and not judge me too harshly.

With this letter I venture to send you a short article published in 1880, not because I think that it possesses any value, because your adjustable jackets are much simpler and more practical, but I wish to show you that I have always felt a keen interest in your method of treatment, which I consider a great advance on previous methods.

That I have, however, used portable plaster jackets only in a few cases during the last ten to twelve years is owing principally to reasons which in themselves have but slight connection with your method.

These reasons I will endeavor to give you briefly.

There is no necessity for me to enlarge at present upon the advantages of your beautiful plaster jackets, which are as familiar to yourself as to me. They are readily applied, are light, porous, cheap, obtainable anywhere, and—a fact upon which I lay as much stress as you do—they enable the physician to treat the case personally from beginning to end.

My reasons for adopting other styles of jackets in spite of all this are as follows:

In spite of all my efforts I could not succeed in giving the plaster jackets that durability which many of my patients required. A large proportion of my patients I see but once a year, some of them but once in two or three years, or not even so often as this, as they do not live in Berlin, and not a few—especially the older ones—require that their jackets should last several years.

The plaster jackets, like all which are of unyielding material, do not allow of sufficient discrimination; in slight or severe scoliosis the same jacket is always employed. As I treat even the mild cases with jackets, as well as gymnastics and massage, and consider the use of the jacket as the most important part of the treatment, it becomes necessary for me to have jackets of varied construction to suit special cases. In slight scoliosis I use simpler forms, which are, of course, less constraining. In severe scoliosis and spondylitis I use more complicated forms.

Elderly people, especially women, feel very uncomfortable in the stiff jackets; they complain of pressure, impeded breathing, etc., inconveniences which can be avoided by the use of steel-drill[1] jackets without losing the advantages of a sufficient support to the body.

Very young women often assert that the plaster jackets, and those of felt and wood, disfigure them more than the scoliosis itself, greatly increasing the size of the waist; further, they find them especially unpleasant in dancing. These disadvantages are not so manifest in the steel-drill jackets.

From my own observation I am of the opinion that the muscular tissue wastes away more quickly under the rigid material than under the steel-drill jackets.

[1] That is, cloth supported by steel bands.

Therefore I now use the plaster jackets only among the poorer classes, or in cases of spondylitis where a good support is required, until a suitable felt jacket can be provided.

In cases of severe scoliosis of little children, as also in spondylitis—in acute stages of the latter as a temporary means—I use felt-steel jackets made on a plaster model and working in exactly the same way as your plaster jackets. So soon as the pelvis is sufficiently developed to give a proper support, I prefer to use in spondylitis the steel-drill jackets with hip attachments and firm arm supports.

Partial suspension I make use of in preparing the models of felt-steel corsets and also as a gymnastic exercise in the treatment of scoliosis. For the latter purpose it is widely used in Germany.

Begging once more that you will kindly pardon my neglect, and that you will remember me most cordially to Dr. Reginald Sayre, I remain with much esteem and best wishes for the new year,

Yours most truly,

FLOR. BEELY.

ODESSA, Dec. 23, 1894.

DEAR SIR:—In answer to your kind letter I communicate you what follows:

In the treatment of spondylitis and scoliosis I consider portative apparatus as a very important factor.

In the beginning of my six-years-long practice I used to employ with success in the polyclinical cases the immovable plaster-of-Paris corsets; in private practice, when the patient's means allowed it, I applied wooden stays. Lately, in the first above-mentioned case I continue to use the same corsets, while in the last I apply Hessing's stuff-stays.

I use the plaster-of-Paris stays in the following way:

Partial suspension after "Beely," with that difference, that at Beely's the patients touch earth only with the tip-toes, while I find it sufficient when they touch it swiftly with the whole sole. On the body of the patient, done over with vaseline or covered with thin jersey, I apply a slight layer of ordinary richly gypsumed bandages of starched stiff gauze (not above two millimetres). When,

after one or two minutes, that layer is congealed, I do it over again on stays using bandages of thick-starched stiff gauze with little gypsum. When the layer has attained the thickness of three to four millimetres and is hard, I take off the whole by making a slit in front and immediately take out from inside the first thin layer, which parts very easily, as it is separated from the plaster-of-Paris corset itself by a layer of vaseline.

The advantages of the method are:

1. The absence of folets on the inner part of the stays. 2. On the hardened surface the stays become firmer, thinner, neater.

After having been dried in the drying room, or in summer in the sun, such stays fit exactly the body, as they contract while dried.

The dry stays are covered inside and outside with jersey by means of starch gum, their edges finished with dogskin; the stay lace is sewed on, etc.

The technique of wooden stays is explained in the herewith joined article. The technique of Hessing's stays is described, as you know, in the *Lehrbuch der orthopäd. Chir.*, von Albert Hoffa.

In the present time I use the plaster-of-Paris stays only when Hessing's stays are inaccessible for the patient on account of their price. The fixation of Hessing's stays is the most perfect, as well in cases of spondylitis as in those of scoliosis. The wearing of stuff stays, bound with steel, is much more agreeable to the patient than that of any hard stays, even the most perfect.

Patients that have worn hard stays, even of the lightest kind—as those of felt, of wood, or of celluloid—when they get Hessing's stays, that weigh much more, assert them to be much lighter, because they lie softly on the body. I consider as the principal defect of all hard stays, not excepting the plaster-of-Paris corsets, that they do *not fit firmly in the waist*. That defect impedes, especially in the treatment of spondylitis of the higher pectoral vertebræ, when the jury-mast is added to the stays, for the fixation of any hard stays on the pelvis is precarious.

As the use of hard stays is inevitable, Hessing's stays being dear, I consider the plaster-of-Paris stays as the best on account of the following reasons:

1. They are cheap. 2. They are easily adapted directly

on the patient without pattern. 3. They do not alter their shape and are not deflected by heat. 4. It is easy by means of a hammer to make a soft place in them. Their solidity, when well executed, is quite sufficient for children, when stays have to be changed every four or five months. They are less convenient when there is no necessity for such frequent change.

Hoping, dear sir, to have answered in a satisfactory way to your kind inquiry, Yours truly,

J. WALTUCH, M. D.

1532 PINE STREET,
PHILADELPHIA, Feb. 14, 1895.

Dr. Lewis A. Sayre:

DEAR DOCTOR:—I may briefly add that my feeling with regard to the use of plaster-of-Paris bandages, in accordance with your suggestions in the treatment of spondylitis and scoliosis, is, that so far as spondylitis is concerned it affords the readiest and best mode of treatment that has ever been suggested for physicians residing at a distance from centers of population, who are unable to avail themselves of the aid of the instrument-maker.

With regard to scoliosis, I have not felt so well assured as to the expediency of using it. In the treatment of this latter affection it is necessary to continue our mechanical support for such a very long period, in order to obtain lasting results, that I have somewhat feared the effect of the necessary compression of the chest on the general development and nutrition of the patient. My theory in the treatment of the latter affection has been to make use of elastic force applied simply in the directions opposed to the deformity.

Looking at the entire question from a more general standpoint, I wish to say that nothing in the modern history of orthopædics has done so much to bring about a revival of interest in this class of cases on the part of the general practitioner, or to conduce to a rational and effective management of spinal affections, not in this country alone, but throughout the world, as your able advocacy of the methods introduced by you.

Yours fraternally,

BENJAMIN LEE.

DISCUSSION.

DR. V. P. GIBNEY.—I simply wish to say that I belong to that class of men referred to by Dr. Sayre who are in favor of treating spondylitis and scoliosis by means of plaster of Paris, and I have nothing whatever to say against its use. I apply this method of treatment both in public and private practice, and I believe that to-day we have no other plastic agent so universally serviceable as plaster of Paris. I use it not only in Pott's disease, but also in joint diseases.

DR. A. B. JUDSON.—I desire to add my contribution to the fund of respect which Dr. Sayre has exacted from the world by his labor and enthusiasm during a professional life of over half a century. The plaster-of-Paris jacket has had great popularity and success. It has brought relief in an important degree to a large number of patients, and has been a welcome gift to the physician confronted with a case of Pott's disease and remote from an instrument-maker. Before the demonstration made by Dr. Sayre, a physician so situated was unable to give mechanical support to the diseased spine, but with the plaster-of-Paris jacket he could treat the patient himself and at once. This jacket, however, does not allow the fine adjustments which are so desirable in the progress of a case of Pott's disease. Its great merit is that it can be applied easily and quickly. But the nature of this disease and the difficulty of its mechanical treatment justify the lavish expenditure of time and attention. The careful adjustments which we can make with soft steel enable us to give the necessary coercion to the bones of the spine, and to preserve to the full the comfort and convenience of the patient. We often hear aluminum spoken of as the coming metal for orthopædic uses, but while waiting for it we may well be glad that we have the commercial steel of the present day, whose fine qualities were unknown ten, or even five, years ago.

Dr. W. R. TOWNSEND.—I desire to thank Dr. Sayre for the pleasure of listening to his paper, and I heartily subscribe to what he has said regarding the value of the plaster-of-Paris jacket. In the majority of cases it is the best possible dressing. I think that most of the objections that have been made against it are due to its improper application. The objection made that it produces excoria-

tions of the skin amounts to nothing, for if the skin is clean before the plaster is applied, and the jacket properly made, such excoriations will not occur. I do not think, however, that plaster of Paris is the best dressing in all cases. When the disease is low down in the lumbar spine, I prefer steel braces. In the future, when the item of expense in connection with aluminum has been overcome, I think we will have at our command a dressing superior to plaster of Paris, as far as weight is concerned. Jackets made of wood and other materials are also very satisfactory in certain cases; the objection that they hold the secretions within has been overcome by puncturing them. Steel braces, properly applied, are also in some instances superior to plaster of Paris; but to properly apply a steel brace requires much practice and the assistance of a skilled mechanic to make it. It may produce good results in the hands of the specialist, but may do much harm when improperly applied. In scoliosis I do not use the plaster jacket very much, but when I do I follow the method devised by Dr. Sayre. To the general practitioner the treatment of patients with Pott's disease by means of plaster-of-Paris has certainly been a great boon and it has also been of great relief to the patients.

Dr. H. L. Taylor.—Professor Sayre has perfected a method which places the treatment of Pott's disease within the reach of all, and it is unquestionable that thousands of sufferers have been relieved by means of the plaster-of-Paris dressing who would otherwise have been abandoned to their fate.

Sufferers from spondylitis require general and local hygienic measures to restore health and promote healing of the tubercular focus. The most important local hygienic measure is some form of protecting and supporting apparatus, which may be a jacket, encircling the body, or a brace exerting pressure at points selected to produce a maximum of effect with a minimum of discomfort. In either case it is my belief that the action of the appliance is mainly that of antero-posterior leverage, transferring the weight from the bodies of the vertebrae to their articular processes. Bradford and Lovett say, in their standard work on orthopedic surgery, that "the undoubted beneficial effect "of plaster jackets is due, not to the separation of the affect-

"ed vertebræ, but to a fixation support in an improved posi-
"tion." * * * "Unfortunately, however, the plaster jacket
"does not of itself, by its hold upon the thorax, maintain a
"continued extension, but the jacket and thorax so adapt
"themselves to each other that active suspension ceases.
"The jacket, however, does act as an antero-posterior sup-
"port, until it becomes loose and inefficient."

I prefer, as do these authors, in most cases, if I understand their practice, a properly adapted steel leverage apparatus to jackets of any make or material, on account of its greater precision, adjustability, and cleanliness; but it should not be overlooked that as regards results the workman is more important than his tool, and that better results will be obtained with a jacket in skilful hands than with the most perfect apparatus carelessly or unintelligently used.

In Young's Orthopedic Surgery, the latest work on the subject, the opinion is expressed that the disadvantages of the plaster dressing in Pott's disease are such as to "relegate it and its modifications to a secondary position." That it is a useful method in public and charity work, in exceptional cases, and in remote districts, but not the best in skilled hands and under favoring conditions, most American orthopedists of large experience are substantially agreed. The dissenting opinions are mainly those of operative surgeons and others who have had little or no experience in scientific bracing with steel supports; and the drift of professional opinion, as reflected in medical literature and in the best practice, appears to be tending in the same direction. It is also to be remarked that in spite of the large number of cases treated by this method in the last twenty years, accurate reports of the ultimate results of treatment five or ten years after discarding the jacket are still wanting.

The value of partial suspension and jackets in the treatment of lateral curvature would require separate discussion, but I shall only remark that there is less support among specialists and in the profession at large for the jacket treatment in this affection than in spondylitis, and that the trend of opinion is largely away from it. Young, in his recently published work, states that "the plaster-of-Paris jacket, myotomy, tenotomy, and forcible restoration under anæsthesia have, in the writer's (his) opinion, no place in the treatment of lateral curvature."

These matters of method and detail are, however, of less importance than the fundamental idea that spinal affections are curable, when the mechanical conditions presented are clearly recognized and adequately met, and nothing has done more to convince the profession of the truth of this proposition than Prof. Sayre's vigorous and tireless advocacy of positive mechanical treatment.

DR. ROYAL WHITMAN.—I am convinced that the plaster-of-Paris jacket is perfectly efficacious and often the best means of treatment in the cases referred to in Dr. Sayre's paper. There are occasions when steel braces or other methods of treatment are preferable, but I have no doubt that properly applied, the plaster-of-Paris jacket would answer in most instances. I do not think this method of treatment will ever be generally superseded, not at least for poor patients, or those for whom complicated or expensive apparatus is for one reason or another impracticable.

DR. SAYRE (closing the discussion).—In closing I wish to say in reply to Dr. Taylor, that only as long ago as last Tuesday, New Year's day, I received letters from three old patients of mine who were cured of spondylitis by means of the plaster-of-Paris jacket.

Stated Meeting, January 17th, 1895.

THE WORK OF THE ACADEMY OF MEDICINE.

VALEDICTORY ADDRESS OF THE RETIRING PRESIDENT.

By D. B. St. JOHN ROOSA, M. D., LL. D.

Fellows of the New York Academy of Medicine:

GENTLEMEN:—Two years ago, by virtue of your favor and choice, I assumed the office of President of this Academy. Since that time thirty-four regular meetings have been held, including the one at which we are now assembled, and seven special meetings. It has been my privilege to attend all of the regular meetings, and to be present throughout their proceedings, in all instances except two, where I was obliged to absent myself after the session was opened. Of the special meetings, I have been present at all but one. I allude to these in themselves unimportant details, because they furnish, if rightly considered, a broad view of the general public work of the Academy, although they can give no idea of the various meetings in which your executive officers are gathered together to consider the machinery which enables the Institution to carry on its work; nor do these facts include the important matters of the various sections, comprising the whole domain of medicine and surgery, into which the Academy is now divided. But the public gatherings of the whole Academy furnish a course of medical instruction which, from practical experience, I am able thoroughly to appreciate. It has been a course of well-considered lectures from experts and authorities on the different topics which they have discussed, and added to this, in most instances, we have also enjoyed the criticisms and suggestions of those doctors equally expert with the author of the

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paper. These papers, with the exception of one from Washington, two from Buffalo, and one from Albany, have been by the Resident Fellows of the Academy. If there ever is danger to any specialist that he tread too closely the narrow track of his own path, such a course as that which I have had the honor of attending, would serve him in good stead to keep him from a circumscribed and cloudy view. I congratulate the Academy on the record it has made in these scientific productions, and in the work of these special meetings. Although we have not succeeded in accomplishing all that we desire, either in the direction of national quarantine, or improvement of the water-shed, and the consequent purification of the water-supply, we have performed a fundamental work upon which a future edifice of progress will be erected. We have turned public opinion in the profession to these important subjects—in the former case, the opinion of the profession throughout the country; and while in the latter, perhaps, the subject does not invite so large a view, we have here stimulated inquiry in a proper way among the profession in this city and state. We have demonstrated the absurdity of trying to purify the water-supply of a city by taking possession of the land lying upon the rivers which furnish it. Besides all this, by your action in respect to both these matters, you have vindicated the right of the Academy to be something more than a mere academic debating club, discussing strictly hygienic and therapeutic questions, without looking for any practical outcome as a result of the debates.

Under the direction of the Council, I appointed a committee to look after the interests of medical legislation in the constitutional convention. This committee, consisting of Drs. Daniel Lewis, Fox, and Pryor, did excellent work, the chief of which fell upon Dr. Pryor, as secretary of the committee. He visited Albany, attended several sessions of the convention, and so impressed committees having matters pertaining to our work in the state in charge, that a

substantial advance was made. It is now possible to secure appropriate legislation in the coroners' system, and in other affairs of which their report speaks very fully. It is seen that the committee was fully alive to the importance of the work entrusted to it. The unanswerable argument, to those who object to bodies like ours undertaking to exercise an influence in higher politics, is that, unless we do, individuals, acting upon their own responsibility, will achieve very much in politics of a lower order. The Academy of Medicine, as I ventured to state in my inaugural address, is not, like the Academy of Medicine of Paris, a collection of mere savants, but it is a body of practical physicians and surgeons, who are obliged, in the absence of state arrangements for medical affairs, to attend to these matters themselves.

In my opinion, the Council of the Academy ought to keep a very strict watch of proposed legislation in Albany, affecting the health of the state, and interfere, as a body, when that is threatened by improper bills, of which it is intended to make laws. The majority, and, I trust, a very large majority of the Fellows of the Academy, believe that, in a broad and catholic way, we should stand between the community and the government, to assist and direct them in all legislation that may affect the public health. This is the only medical body in this city which can at all adequately perform this work, for the simple reason that the term of office of the executive officers is long enough to allow plans to be made and carried on, without interruption or haste, for a reasonable period of time.

During these two years an attempt was honestly made to convert the beautiful rooms on the lower floor to club purposes, for the benefit, solely, of the Fellows, that they might cultivate the social amenities with each other, when not engaged in the meetings of the Academy, the sections, or the committees. This attempt has not been entirely successful, but I believe the good judgment of the Academy

will finally evolve a way in which the smoking-room and dining-room may have a larger use than that to be obtained immediately after the stated meetings, and that a wider acquaintance among the members will there be cultivated.

During the last year the library has been enlarged by the accession of four thousand volumes and two thousand five hundred pamphlets, making a total number of fifty-one thousand volumes; and the space for the library has been increased to such an extent, that we may believe that it will be sufficient for the uses of the Academy for the next decade or two, unless the generosity of our fellow-citizens and of the Academicians may be so great as to exceed our present ideas. The Academy deserves the largest medical library in the United States. New York, whatever may be thought of Washington and other centres, is the place where such a library will accomplish the most, where it would be the most accessible to the largest number of workers; and, I have no doubt that a future president will inform you that that end has been accomplished, and that the greatest reference library in this country is to be found within our walls.

A minor improvement, that has occurred during the past two years, is the establishment of our own plant for the generation of electric light, so that the expense of lighting our building will be notably decreased.

The formation of the Bureau for Nurses, and its successful establishment during the past year, is a feature for which the Fellows are, I am sure, grateful, and which they will desire to amplify and sustain in every possible way. For the success of this bureau we are indebted to a very able committee, of which Dr. Landon Carter Gray has been chairman.

During my term, a law has been passed by the Legislature of the State of New York, which exempts such institutions as ours from taxation. This, certainly, the state ought to have done long ago, and not have compelled the

Academy to make certain regulations in regard to a public library before it could be exempt from taxation. The work of this Academy is actually work for the state and the community. There are no private interests to be served here, and I have no doubt, unless taxation is made general on every church, hospital, reformatory, and similar institution of the country, that this righteous law will never be repealed. I took a very great interest in helping to originate the movement which finally ended in this general bill, and, in conjunction with members of the legal profession and managers of certain benevolent institutions, assisted to the extent of my ability in carrying the work through.

On my accession to the presidency, there were seven hundred and fifty-two Resident Fellows, thirty-two Non-resident Fellows, forty-two Corresponding Fellows, and nine Honorary Fellows, being a total of eight hundred and thirty-five. At the close of my term, in 1895, there are seven hundred and seventy-four Resident Fellows, sixty Non-resident Fellows, forty Corresponding Fellows, and seven Honorary Fellows, making a total of eight hundred and eighty-one, being an increase in the whole number of forty-six. The annual report of the treasurer, printed and circulated with the notices for the annual meeting, for the benefit of the Academy, for the first time after my term of office began, in the year 1894, again renewed this year, gives you an accurate idea of the Academy's financial condition, which is thoroughly sound, although, we may hope, when the present period of commercial distress is fully recovered from, that the gifts of generous men, who appreciate our work, will make it much better. This period has been so severe, that young members of the profession, fully eligible, whom we should have been glad to welcome, have hesitated about incurring the obligation of the admission fee and the annual dues. It is a matter of pleasant surprise to me that we have decidedly increased in our resident fellowship, in spite of the difficulties induced

by the stringency in the money market. But, I have no doubt, more prosperous times will also bring prosperity in fuller measure to a profession which, although it serves the public very largely, without honorarium or fee, is among the first to suffer from any general financial depression.

At this point I think we have a right to indulge in a little self-congratulation over the business capacity of members of the medical profession. It is sometimes asserted, and perhaps believed, that our profession has no aptitude for business affairs. I have always thought that our apparent want of business skill for private matters was simply a want of attention, generally enforced by the exactions of professional life, to the details that are necessary to make business thriving—in other words, a man usually has no time to practise medicine and to also make money; but that a man with intelligence and education enough to make a good doctor, would generally make a good business man, did he attempt it, I feel morally sure. At any rate, let us look at the facts in regard to the business outcome of the labors of the founders, and benefactors, and previous officers of the New York Academy of Medicine. We are now, after a period of forty-eight years, in a commodious, fire-proof building of our own, which cost, in round numbers, about $375,000, in the heart of a beautiful part of the city, having begun in a very humble room, in an insignificant building, in an obscure quarter of the town, with no library and no funds. We own all the furniture, and the library, which, of course, has a value not easily estimated, and we owe at this moment, say, in round numbers, $13,000. If any of our business gentlemen, or artists, or lawyers, choose to make a comparison with the condition of their club-houses, or institutions devoted to professional work alone, I am sure we shall not shrink from it. The capacity of physicians to govern hospitals was demonstrated by the Medical Corps of the United

States Army and Navy during the Civil War. Never were hospitals better managed, and in them none but doctors had a voice. The present financial condition of our Academy is a proof that this aptitude for business affairs extends to such an institution as this. We may, I think, invite the most careful investigations of our financial methods, without any fear as to the results of the examination. These statements I think pretty accurately represent the work of the Academy during the past two years, as far as it can be represented by statistics and figures. Whether the spirit and aims of the Academy, during that period, have been of a high order and worthy of the profession which it represents, it must be for your judgment to determine. Certainly, it will be a matter of most profound regret to the one who now gives up the position, which he has held during that period, if it should be your conclusion that, in any manner, the aims of those who have founded and maintained this institution, in a broad spirit, shall not have been respected and amplified, so far as changed conditions rendered this possible.

The Academy is no longer a small body of physicians, in a provincial town, but it is composed of nearly a thousand members, in a metropolis which is becoming justly celebrated for the achievements of its Fellows in medical science and art. We may claim for New York that it is one of the medical centers of the world, fast assuming an importance which may cause it to be placed on the same plane with those that have existed for centuries in the Old World, and it may be said to represent that large body of men and women called the medical profession of the city.

I have been greatly honored by the suggestions made by various Fellows of the Academy, whose opinion I highly regard, and by the nomination, made in open meeting by one of the Trustees, that I should again become a candidate for your suffrages. But, from the moment that I received the news that I had been elected until now, I

have never changed my purpose—not to seek a re-election at your hands. There may have been times in which, during the formation of the Academy and, especially, in the erection of the new building and preparations for it, when it was wise to secure the services of a president for two or more continuous terms, from among the eminent men whom you have elected to preside over your deliberations. But that period has passed. The honor, in my judgment, of being President of the Academy, is very great—perhaps second to none that can be obtained by a member of the profession in this country. As such, it is worthy of most grateful acceptance when conferred, but it is an honor that ought to be distributed during a decade, at least the number of times allowed by each president having but one term. No good purpose can now, in my opinion, be gained by two continuous terms. The period of service is long enough to allow of the maturing and development of plans, if the President has any, for the building-up of the Academy, and if such an officer be sustained by the Council and Trustees.

Before I turn to welcome my successor, and to hand to him the insignia of office, there are one or two minor points, in the way of changes in our management, which I would respectfully propose for your consideration. I am very well aware that the views of a retiring president cannot have the importance of those of his successor, and yet there may be a little value attached to the experience of one who has been your chief servant for two years. In my judgment, it would be of great service to the Academy, if the offices of Treasurer and Treasurer of the Board of Trustees, were merged in one. The present system produces an unnecessary want of clearness in the statement of accounts, and diminishes the working capacity of both the Council and the Trustees. The Academy should also ask the legislature for an amendment to its charter, in order that it may have the power to hold more property than is

at present conferred; and, in general terms, I may say that I believe the present constitution and by-laws, in a few instances, have been outgrown, and need modification and simplification.

The Academy has chosen for its new President a gentlemen well-known to the profession in this city and country. He has held high executive positions, and entirely to the satisfaction of the profession. He is in the flower of life, in the full vigor of manhood, and, I believe, adequately represents the desire of this Academy to become a great power in conserving the health of the commonwealth, and in advancing the scientific knowledge of our profession. For the past year, as Vice-President, he has, by his regular attendance at the meetings of the Council, come into very close relations with the governing body, has thoroughly learned the workings of the Academy, and is in full sympathy with its aims. I congratulate the profession on his accession to the position, and I predict for him a most useful period of service, which will always be gratefully remembered.

And now, thanking you for the consideration which has been extended to me, as I have endeavored to do my duty in this office, I turn to my successor with interest and pride.

THE INAUGURAL ADDRESS OF THE PRESIDENT OF THE NEW YORK ACADEMY OF MEDICINE.

BY JOSEPH D. BRYANT, M. D.

The foremost duty of one who is honored by his fellowmen is to express his grateful appreciation of the manifestation of confidence and esteem, and to pledge his earnest thought and conscientious labor to the support of the best interests of the charge intrusted by them to his guidance and care.

The fact that you have deemed it wise to honor me, and, too, out of proportion to my merit and expectation, by placing me at the helm in the affairs of the Academy of Medicine, is a tribute of regard and trust which, while it emphasizes the value of past conscientious effort, none the less admonishes one of the necessity of a continuous and untiring endeavor.

Indeed, I thank you sincerely for this earnest of your confidence, and beg to express the hope that no future official action of mine will cause the lodgment of a well-founded regret in the mind of any one.

The usages of the occasion that now claims our attention dictate that a careful consideration shall be given to the affairs of the organization to which we belong and to the advancement of whose prosperity and strength each of us should now pledge an unreserved and loyal support. The attainment of this desideratum is made easy when earnest effort and honest self-denial and abnegation characterize the labor and attempts of men who are engaged in the exaltation and advancement of a common cause.

But it should not be forgotten at the outset that decided differences in honest opinions and in upright purposes are

the inevitable products of earnest discussion, when engaged in by sincere and well-meaning persons, whether they are the prime factors of scientific bodies or those of mighty nations. And, whichever they are, the results of deliberate conclusions, when secured by fair means, demand alike the sincere support of all who regard with proper thought the supreme importance of wise control, good order, and well-founded prosperity.

The endurance and thrift of every organization depend on the honesty, the harmony, and the productiveness of the administration of its affairs.

The Academy of Medicine is a definite and well-established organization in every sense of the term, and, like all similar ones, has assets of a material and, if you please, of immaterial nature as well, whose value and productiveness are amenable to the same influences as those of organizations of a baser nature.

The marketable value of the structural, landed, and monetary investments constitutes the common material foundation of this and other well-organized and enduring bodies of men; not only because each member of the organization exercises an inherent right in the control, but but also because the members themselves can find culture, or comfort, or good-fellowship in connection therewith whenever the inclination or opportunity to do so may arise.

Nor is this all in the instance of the Academy of Medicine, since within the structural possessions are stored the means of mental enrichment and professional exaltation which not only serve to stimulate and increase the fruitfulness of labor, but also beget opportunities and examples that attract to the field fresher energies, greater efforts, and increased prosperity, all of which are well exemplified by two significant words—increasing membership.

Unfortunate indeed it is when for any reason the worm of discontent or the canker of indifference fastens itself on

the energy or loyalty of the members. Then, material assets of an organization—the landed, structural, and monetary; books, the means of mental enrichment; and membership, the basis of existence—all suffer from the dire effect of the unheeded influences of time, temper, and tempest.

The present condition of the common material belongings of the Academy is well stated already in the usual reports bearing on these subjects. However, there are two other belongings, or assets, if you please, to which I desire to call your attention, because each should be cultivated with care and treated with circumspection and forethought in an equal degree.

I refer to the books—the library,—and to the Fellows—the members of the Academy. The library should be regarded as a legacy from the present to the future of the Academy, and from the present to the future of medical achievement, and indomitable energy should be wisely exercised in the various channels of activity to secure suitable amounts of money, the income of which shall maintain the library at as high a state of efficiency as may be required to keep pace with medical attainment.

Fellows of the Academy come and go, live, die, and are forgotten, except in the evanescent recollections of those who are near to them in the rank of departure. But good books—"the monuments of vanished minds"—are, as Milton said, "the precious life-blood of master spirits, embalmed and treasured up on purpose to a life beyond."

The larger and more comprehensive the library becomes, the greater and the more enduring will be the fame of the Academy, and the more beneficent will be its influence on all things coming within the meridian of its activity.

There is not a member of the Academy who can not with ease and without personal hardship garner an amount, no matter how modest, which will add the weight of its influence to the force necessary to maintain the library at a

maximum state of efficiency. The yearly report shows the membership of the Academy to be above seven hundred, and still exhibiting a healthy increase. Now, surely, if each of the seven hundred were to contribute an average of but one hundred dollars to the library fund, the income of the aggregate sum, added to other means now in our possession, would not only meet the wise demands of the library, but also, in so doing, would enable the management to lessen much the yearly tax that is now imposed on the Fellows of the Academy.

While it is true, of course, that there are Fellows for whom the contribution of a hundred dollars would constitute a genuine hardship, yet it is equally true that by far the greater number can each contribute twice or thrice that amount without the least well-founded inconvenience. It is a pure question of personal patriotism.

Those who are not blessed with sufficient funds to make contributions that are consonant with their sense of pride should not forget the facts that patriotic endeavor will bring prompt and cheerful aid from other than personal sources, and, too, that the "widow's mite" is as expressive of earnest, honest intent as the munificent contributions of plethoric resources.

No matter what else may be done to meet this end, let each member constitute himself a committee of one for the purpose of adding his part to a fund that shall glorify his name, not in proportion to the amount given, but in proportion to the cheerfulness and ability of the giving.

All requests for aid and support should be made with becoming dignity and couched in terms that shall impress the fact that he who contributes to medical attainment gives to mercy's cause.

While it is true that the library is the chief vital force of the Academy, and that its healthful influence is essential for a proper growth of the organization, still, it is very necessary that the obligations of the Academy to other demands

be carefully considered as they arise. All financial exactions should be anticipated, provided for, and scrupulously met; moneyed obligations should not be incurred in advance of the ability of their suitable fulfilment; advice on sanitary public questions should be willingly though discreetly given. The desire to be as wise and thoughtful citizens as physicians should be welcomed and regarded as an earnest of increasing patriotism and conclusive of enduring national pre-eminence.

It should not be forgotten at this age and day that a reputation only for medical lore ill rewards one for the heritage of a dwarfed sentiment or the palsied action bred of indifference or inattention to the common requirements of human obligation.

I can not at this time disregard the fact that, although we are members of a learned profession, and for this reason are believed to be in the enjoyment of established prosperity, there are some of our members who find it difficult to meet their annual obligations to the Academy. For such as these I have the sincerest sympathy, and bespeak for them the indulgence and aid of the more fortunate members of the organization. Their burdens will be lessened in direct proportion to the magnitude of the contributions of those who are more favored and to the conduct of affairs with a suitable degree of economy.

A righteous consideration of the sense of duty and the rights of the taxpayers of the Academy demand that no unnecessary or prodigal expenditure of the Academy's funds shall be permitted, that every avenue of unremunerative outlay shall be closed, and that all indulgences that contemplate an unproductive drain on the treasury or a lessening of the dignity of the organization shall be promptly prohibited. So far as I myself am concerned, I am not in favor of any new departure or the maintenance of an old one that implies an unrequired drain on the treasury or the standing of the Academy and its members.

The prospective products of the exercise of wise conceptions and well-directed mental endeavor constitute the immaterial expectancy of every scientific organization that adds facts to fancy, proof to conjecture, and excellence to mediocrity. If this is true, then it is easy to understand that for the achievement of success and the maintenance of good-fellowship both the material and immaterial factors of a body must be attuned in harmonious accord.

In this organization it should be recognized and admitted at the outset that the chief aim of wise conceptions and of mental endeavor contemplates personal unity and improvement and the advancement of medical science; and that any ideas of policy or duty that harbor intentions prospective of doubt or dissension in any degree should be conceived with care, considered with deliberation, and enforced only after a patient heed has been given to the opinions and rights of all others who may be equally concerned.

I will not consume needless time in the recital of details, but, instead, will declare at once that I am well aware of the fact that increased honor is ever attended with increased labor and responsibility.

I shall regard it as my duty, and it certainly will be a pleasure, to labor untiringly in the support of every just and proper measure that emanates from within the organization. It will be my duty to place matters before you in such a manner as to enable you to act with proper understanding and without the danger of subsequent regret. It will be my duty also to hold the scales of deliberation in proper adjustment, in order that our business proceedings shall be characterized by the harmony, justice, and dignity that comport with our station in affairs.

Such, indeed, are but a few of the self-evident duties. Numberless others, many of which are yet in the future, and, perhaps, all unforeseen, will come unbidden and possibly unwelcome upon us. However this may be, it is to

be hoped that we shall bear patiently all disappointments and distrust, and earnestly seek for the rewards of constancy, consistency, and courtesy. At all events, I wish it understood that I regard myself as a servant, and not as the master in your affairs.

May I not now bespeak your co-operation and support in all matters that are just and equitable? In all others than these I shall deserve your opposition.

It is to be regretted that the attendance at the Academy meetings is not greater. A prompt and large attendance will beget a deeper and more lasting interest, foster a broader and more intelligent appreciation of the Academy's affairs, and correspondingly increase the *esprit de corps* of the members.

Finally, I desire that we bear with each other patiently at all times, being assured that all differences will be honest differences, and all conclusions both sincere and just.

Mr. President, in accepting from your hands the great trust which you have so zealously guarded, I comprehend very fully indeed the responsibilities that are incurred. Responsibilities which under ordinary circumstances one might accept with little fear are made doubly imposing now by the coming from a master hand.

Yet I am cheered by the thought that the results of the earnest effort and honest endeavor which you have exhibited may be duplicated by another, if he is wisely moved by the sense of duty and the power of earnest example.

Surely the task is made great by the knowledge that this organization has been fostered by the generous Purple, the genial Barker, and the scholarly Jacobi, and the sagacious Loomis, and stimulated by the restless vigor of a Roosa.

If at the end of the term we can point to increased facilities, increased endowments, increased membership,

and increased good-fellowship, then indeed shall I be gratified, and feel that I have not lived in vain, and that I may exclaim, "Ye gave unto me this which I now return increased many fold."

Stated Meeting, February 7th, 1895.

SOME REFLECTIONS UPON THERAPEUTICS AND THE MATERIA MEDICA.

By E. G. JANEWAY, M. D.

It has seemed to the speaker that a discussion upon some points of our materia medica taken in its broad sense might be of interest, even if it should not prove productive of benefit in bringing about a harmony of statement upon important points in lectures and books. There are, no doubt, those who entertain fixed opinions upon the question of interfering with the accumulated mass of remedies, and of preparations of these remedies, believing that as some may want to use this and some that preparation, it is well to have all retained. Alas for the medical student of to-day who has had a large addition to his labors in the study of bacteriology, he can appreciate the value in time and mental effort could it be possible to abbreviate the amount of compulsory knowledge of materia medica without essentially crippling his resources in his conflict with disease.

The question which has come at many times forcibly to myself as student, teacher, and as practitioner has been whether it would not be possible to reduce the number of preparations of drugs which it was compulsory to study. Moreover, this additional question has forced itself upon me: Would not such a measure be of benefit by allowing the memory to fix more tenaciously upon the physiological properties and the therapeutic indications of drugs when less time is required to master the dose of a drug and of the different preparations? In order to bring about such a

plan, the speaker would suggest that the materia medica, as arranged for students, be so divided that the dose of the necessary preparations should be given in the text with the description as at present; and that other officinal or non-officinal preparations should be given either in parentheses or separated by lines from the other, or in an appendix. In this way eventually there would, in my judgment, be brought about an elimination of the unnecessary. Students are not inclined to learn more than is absolutely required, and pharmacists would not prepare what was no longer called for.

But here we strike the point at issue, who is to decide what is necessary and what unnecessary? The speaker's suggestion would be that teachers of materia medica and of the practice of medicine in our different medical colleges should, by conference and correspondence with one another and with the teachers of special branches, settle upon the basis of an agreement. The first question that should present itself for discussion would be: Are any of the drugs now learned of so little utility that they might better be discarded or assigned to the appendix, or supplementary materia medica as we might designate the non-compulsory portion? There is no doubt in the mind of the speaker that if each of us should candidly go over the list of drugs which he was accustomed to use, and if all would be satisfied with the selective pruning of one, that a radical reconstruction of the materia medica upon a very simple basis would be the result. Each of us could name quite a list of drugs which he was no longer in the habit of employing. The writer will confess that since he left the hospital he has never employed asafœdita, and much as some may like certain of its qualities he has yet to see a case supposed to call for its employment which will not improve by the use of other measures. Take again such drugs as sarsaparilla and mezereon. Much as has been the popularity of the former, and to a certain extent of the

latter as an adjuvant in certain disorders, it is the writer's belief that they are not necessary medicines. Much of such reputation as they have had has not been dependent upon their activity, but much rather upon the admixture with them of drugs potent in their effects for the specific purposes desired, as iodide of potassium. The writer certainly for years has been in the habit of using the iodide, whether of potassium or of other salt, in saturated solution dropped into milk or water, believing that the readiness with which the dose can be changed makes this far preferable. Hence he has, as above stated, allowed sarsaparilla so far as he is concerned to go into disuse. He believes that many do as he does. One of the aims of this evening's discussion, involving so many of the Fellows of the Academy as have been invited to be present, is to find out whether there is enough of community of feeling to lead us to become iconoclastic or not.

The writer could enumerate a number of other drugs which he rarely or never employs, amongst which are the preparations of antimony, though there was a time when it would have seemed to some a heresy to suppose that a person could recover from a cold without its use. In no direction will the newer therapeutics, as in the use of a remedy like antitoxin, become of more service than in the study of its production. The horse illustrates the *vis medicatrix naturæ*. He becomes immunized after a time by the development of antitoxin from nature's laboratory without the use of a long list of tonics, blood alteratives, and the like. In fact, good food and care allow him to accomplish for the profession the beneficent result out of his own flesh and blood, by metabolism from oats, hay, and water. The writer has no doubt that very useful studies may and will be made regarding those agencies which will tend to hasten or retard this internal change through the study of the effect of different medicinal or other agencies upon the development of antitoxin. Moreover, he believes that

in this way can be best tested the true position of remedies supposed to be curative of diphtheria.

Again we have a number of vegetable remedies in the group of tonics and bitters, some of which are rarely used. It seems to the writer that a student might well be taught the best of the class, as cinchona, gentian, quassia, and columbo, together with nux vomica and possibly ignatia, which are in reality more useful bitter tonics than those generally classed as such. Here, however, we encounter a consideration which is one of considerable trouble in dealing with materia medica questions. Shall we introduce willow bark in order to speak of the value of salicin? I have never used willow bark and never will, but have been accustomed to use salicin partly as a bitter and in part for anti-fermentative purposes, or for its uses as an anti-rheumatic. Not alone in the bitter tonics, but in each class of the materia medica there is a superabundance of drugs to accomplish the same end. Let us consider anthelmintics. Would it not be as well if the student were taught that santonin and spigelia would destroy the round worm; and that aspidium, kooso, and perhaps pumpkin seed would accomplish the same purpose with the tape-worm? He could refer to the appendix if the worm were extraordinarily refractory. I do not believe that in any other portion of materia medica a multiplicity of drugs is more productive of unsatisfactory results than in tape-worm anthelmintics, owing to the length of time some of them are kept before use. In fact, some of the unprofessional tape-worm extractors make a success because of the freshness of the drugs employed. It is folly to expect druggists to throw away the samples of a drug which they have had for years waiting a prescription. This desultory touching of a broad subject seems to me sufficient to indicate the line of thought in my mind. So far I have only alluded to the old remedies, but the synthetical chemist has been at work, and he is striving to introduce new compounds,

each one better than its predecessor, free from the dangerous tendencies which the last has been found to possess,—a panacea for pain, a safe antipyretic, a natural sleep producing hypnotic without danger of habit formation, an organic compound of iron sure to produce red blood globules and hæmoglobin, and lastly, a non-poisonous antiseptic.

At the same time mention should be made of this fact which is, I believe, an outcome of the multiplicity of drugs recommended for the same effect. A discussion upon the use of ergot for the control of internal hemorrhage, other than uterine, would show a very divided opinion about its efficacy.

There remains another point to be considered which will be raised should an attempt be made to reduce the number of drugs, and that would relate to the subject of toxicology. The question would be asked as to the method which should be adopted to inform the student of all the poisonous agents, their derivation, and antagonisms.

I should prefer leaving the best method to the ingenuity and constructive ability of the professors of materia medica and of toxicology. Wherever else we land in our consideration of the vegetable materia medica we find a reproduction of the same condition of affairs. If we look at that class which might perhaps be called, as Biddle has named it, blenorrhoetics, though some are supposed to have exactly the opposite property, we find uva-ursi, pareira brava, chimaphila, buchu, copaiba, cubebs, santal-wood oil, etc. Cathartics are also present in large numbers,—many of them rarely used or else largely retained because they form a part of the old compound cathartic pill. It is my belief that it would be better if there were no such thing as the old compound cathartic pill. I have made it a practice for a long time never to give calomel in conjunction with another medicine in the same mixture, because of the tendency of people to have them renewed,

but to give it separately at the same time with specific instructions as to future use.

The writer would more especially desire to see a number of the preparations of the different drugs relegated to the supplementary portion of a revised materia medica. Let us take iron for illustration. Every possible combination seems to have been tried and many of them introduced in the officinal list. I do not see why the student's memory should be taxed with the doses of the different varieties. A suitable selection could be made of those most employed. If when the student is engaged in practice he feels handicapped by the brevity of preparations he could pick out some from the supplement. How a vote on which should be retained would result, I am puzzled to know. A guess would give reduced iron, carbonate of iron, sulphate, tincture of the chloride, iodide, hypophosphite, pyrophosphate, possibly the acetate on account of its being an ingredient of Basham's mixture, and my own preference for the ordinary chalybeate effect, the tartrate of iron and potassa.

The per salts would be retained for their astringency, and the hydrated oxide and possibly dialyzed iron for antidotal effect in arsenical poisoning. A long discussion could undoubtedly be had upon which is the best chalybeate preparation. For myself a solution of tartrate of iron and potassa in water with a neutral glycerine added to prevent oxidation has proved superior to the others, or has succeeded where the others had failed in chloro-anæmia sufficiently often to warrant my voting to retain it. The success with this preparation is no chimera; it is non-constipating, not irritant, not destructive to the teeth, and contains potash which chemical analysis shows to be as deficient in the blood of the anæmic as the iron. In comparison with ferratin, one of the new progeny, in some tests it proved decidedly superior while very much cheaper. There is a suspicion in the speaker's mind that

many here have much the same inclination toward some other preparation.

For another illustration let us consider colchicum. Why does a student, or in fact a practitioner, want to remember all the doses of the preparations of the seeds and of the root? What we need is a single solid extract, and a single liquid preparation of colchicum be it called a wine or tincture. It could be made as far as the liquid preparation is concerned from root and seeds, only to be of a definite ascertainable strength. If it is insisted that we should have a fluid extract, let that be of a combination also, or of one only. The multiplicity of preparations has been in my opinion a mistake which should long ago have been obviated. Particularly where alkaloids, which are safely usable, exist, do we deprecate multiple preparations.

Of late another tendency has come to the front, that of employing and recommending some new salt of a well-known remedy. I have never felt the influence of this more strongly than in the case of the bromide preparations. A study of the method of origination, medical introduction, and use of some of these gives a clue to the reason why there are so many remedies. When patients are referred to city physicians, particularly to specialists, it is very hard for the city physician not to have a new remedy which he can recommend as superior to the old, which has already been tried. Hence, in a measure, come recommendations of lithium, calcium, strontium compounds. It is wonderful how, notwithstanding the statements made, the most of us still fall back on the potassium or sodium salt of bromine. What wonderful things, too, were claimed for the strontium salts,—the lactate for illustration, in its influence upon albuminuria. I must confess that it has not proved the success which I had hoped when I read of its encomiums on introduction.

This introduction of new remedies would be very well were the old more thoroughly studied; but when we

read in one of the newest text-books that aconitia should never be used, and yet find that some are making use of it, with good results; when we encounter the dose of atropine in a new text-book as 1-50 to 1-40 of a grain; when we find that the dose of aconitia is given in the "United States Dispensatory" as 1-640 of a grain, we feel that there is need of a revision of knowledge and agreement of statement about some useful remedies of long-ago study. It reminds me of the first use which I made of the sulphate of sparteine. Years ago, a foreign physician asked me to write a prescription for pills of sulphate of sparteine, 1 grain each, for rapid, irregular heart of old age. He claimed that Germain See had given him the remedy in that dose with beneficial result. I did so, but after his departure looked up the "United States Dispensatory" of that date, to find that if its statements were correct I had given him twelve times too big a dose. I was much delighted two weeks later to have him thank me for the aid which the remedy gave him and find marked improvement in his condition. Shortly after this I saw that Nothnagel had found sparteine of little value, but discovered the reason in the small size of the dose he employed.

Why should a student study and be obliged to remember the doses of all the preparations of opium, rhubarb, ipecac, etc.? What it leads to I have already indicated, and can be best seen by taking any skeleton manual of materia medica. Here it will be found that not "survival of the fittest" exists, but what is judged to be necessary in order to pass college, hospital, and state examinations, a list of drugs and preparations of drugs, with the doses, and the briefest also.

The tablet form of administration of drugs has come into much favor, owing to the ease of dispensing a definite dose of the remedy, and to easy carriage of the medicine. But any one who has paid attention to the subject must have been struck by its little adaptability to some remedies which

are put up in this shape. This is especially true of remedies slightly soluble of fairly large dose, and hence sold as made in firmly compressed mass.

Years ago, in looking at the stools of a dysenteric patient I encountered three compressed tablets of quinia sulphate, which had been given to him by his physician on the previous day.

Four years ago a gentleman came to a city hotel to consult me with relation to jaundice and a pain in his right side. The day after his arrival he was taken with a new attack of pain and fever, and I did not see him until the third day. He had in the meantime received treatment for his fever from the physician residing in the hotel at which he stayed. In the examination which I made I became convinced that the source of his trouble was gall-stones, and requested him to carefully sieve his passages, and save and send me in a bottle any firm, solid masses like stones. He brought me on the following day a number of fecal masses. As the time of my clinical lecture was close at hand, I took the bottle with me to show the students the method of washing the masses and looking for the gall-stone. I was surprised to find five whole and two half tablets, altogether unaltered, of the phenacetin, which the physician had given him for his fever. The first was somewhat covered by feces and thought to be a gall-stone, until washed and revealed in its own shape. A pigment calculus was finally met with. I have no doubt that to this day the physician who had charge of the patient ascribes the fall of the fever to the influence of the phenacetin.

Last summer a gentleman from Canada came to me, with a letter of introduction from a fellow-physician, stating his inability to sleep. In inquiring as to the remedies he had used, he stated that fifteen grains of sulphonal had been tried, but proved of no effect. I was hurried that day but told him, on learning that he had used the sulphonal in tablet form, that I would give him the same drug in the same dose, only that he was to take it as a powder stirred in hot broth. He slept peacefully under its influence.

The physician who uses these large, firm tablets of difficultly soluble materials must not be surprised at the lack of success which attends his ministrations, or else like that one who prescribed for the fever of gall-stone colic, ascribe to a remedy the favorable termination of a disease. Some claim that when they give these tablets to a patient they advise him to break them up. Patients will not do this, unless we order a mortar and pestle to accompany the tablets. It seems to me wise to discourage the production of this class of remedies in this form.

Again, it has occurred to me on several occasions to find that a patient is unable to bear one half the dose of an irritating remedy in the form of a soluble tablet triturate, that he can take in a solution. Particularly has the writer found this true as regards corrosive sublimate and biniodide of mercury. The reason is not far to seek. A limited area of the gastric mucous membrane receives the effect of the remedy, whereas in solution it is distributed in a more dilute state over a larger area.

Again, I have discovered another source of trouble with these tablet triturates, and that is the tendency to give larger than the necessary dose of a remedy because the physician happens to have some of a definite strength. Children, too, at times get doses intended for adults. This, however, is not a fault which can be laid to the tablet, but simply that it tends to discourage thought as to the proper dose for a given patient. In how far tablet triturates though soluble are liable through age and exposure to deterioration, I have no means of finding out.

I would like to propose one more question for your consideration, and that relates to the responsibility of the medical profession with reference to the endorsement of mineral springs of no special medicinal value as remedial agencies.

In place of the older alchemists' desire to transmute metals, there seems to exist a very strong desire to trans-

mute water into gold by medical endorsement of this and that spring, possessed of no special medicinal ingredient, as particularly adapted for the cure of a number of diseases. The laity are led to look on the water as curative, not because it contains water, but not infrequently because dubbed with a name implying that it has a chemical composition sufficiently strong to be efficacious, even if the supposed ingredient is present in quantity scarcely larger than to give a spectroscopic evidence of its existence—certainly not enough, if a person should drink a gallon a day, to get such a dose as is ordinarily advised by a physician.

Again, patients at times go to large and, considering their means, unwarrantable expense, in order to take hot-water baths, which prove of no benefit for the form of disease with which they are afflicted. So long as this is done independently of medical advice, no one can object.

It does seem to the speaker as if the medical profession should be especially wary in recommending any water which has been given a name with intention to mislead as to its composition, or whose name does mislead; and it seems to the speaker that some of the advertisements of mineral water accomplishments in the way of asserted activity must be rather annoying to those physicians who have been entrapped into endorsing the water for certain properties.

In traveling we need to know of a potable water upon which we can rely with some degree of certainty that it will be free from liability to produce disease. So, again, waters which are remarkably pure can well be recommended for this property to people living in sections where poor water only is obtainable. Several illustrations have, however, come to the speaker's knowledge, tending to show the danger of not endeavoring to secure as absolute as possible a protection against the sources of danger in the domestic supply. The imported water fails to arrive

owing to some mishap; the servants resort in the emergency to the contaminated home-water, with the result in one case in my experience of producing five cases of typhoid fever, and that in the family of a physician. The discovery of the cause was only brought about by my inquiry into the cause of the typhoid.

There is another danger in this regard, to which my attention has also been drawn, namely, the possibility of substitution of other water for the supposed pure water. In looking for the cause of a case of typhoid fever not long since, I found that the lady who was ill had only drunk as water, for six weeks preceding her attack, a certain very popular water dubbed with a medical name, and peptonized sterilized milk. On examining some of the bottles of this water which awaited consumption there was found quite a scum and sediment in several. No extended examination was made as to the nature of this.

There is no doubt of the value of mineral waters properly used, but it has never seemed to me quite fair to have a patient pay considerable money for something whose only value is water.

It seems at times as if we were liable to treat the name of a disease rather than the condition of a patient. That will, indeed, be a happy time, when, learning the appropriate name for the morbid state, we shall be able to prescribe a remedy. We have but few specifics, and even in the use of these, judgment and a study of the patient's condition are necessary.

Shortly after my entrance into the profession, a fellow-interne in the hospital was stricken with a fever, the supposed cause of which was found in the condition of his urine, which contained blood, albumen, and casts, and the name of his malady was at this time nephritis. He was given podophyllin to keep his bowels relaxed, and was made to take a hot bath each day. At the expiration of ten days of this treatment, an examination showed an

eruption. The name of the disease was changed to typhus fever; the cathartics were discontinued, and in their stead whiskey was ordered. No marked change was noted in his condition to call for the change in the treatment; it was simply dependent upon the mental conception of the requirements of typhus fever then in vogue.

This is an extreme case, and it made a lasting impression. Yet it has often seemed to me that we are influenced by a name in our doing or not doing, to a greater degree, perhaps, than the conditions warrant. There can be no doubt of the efficacy of treating malaria, whenever we can justly name the disease thus, by quinia, nor of making use of mercurials and iodides in combating syphilis in the appropriate stages. In how far we are justified in our apprehensions by the name typhoid in withholding laxatives during convalescence from this disease when constipation is marked, has seemed to me a matter for study.

Again, it has seemed to me that we are too inclined to treat a pneumonia in a certain definite fashion, because it is pneumonia, without considering the requirements of the case. In the early stage of the disease during the chill, I have a belief that vigorous measures might at times abort it, and yet I recognize how difficult it not only is, but will be, to feel sure that one has aborted a pneumonia. However, on the other hand, I feel that the idea that a patient is seized with pneumonia, or probable pneumonia, brings to most physicians the conception of a condition that must progress. The speaker has met elderly people, free from any malarial influence, who, in the late winter, after exposure to chilling conditions several hours previously, have been seized with a chill, with some elevation of temperature and even pain in the side, which has subsided under prompt diaphoretic measures, combined with aconite to control the circulation and followed by a large dose of quinia.

The speaker believes that at a time when pneumonia is unusually prevalent, it would be beneficial if people were instructed the moment a chill began, to institute measures looking to free diaphoresis—one drachm of sweet spirits of nitre and three drachms of liquor ammonia acetatis each hour; a hot mustard foot-bath for twenty minutes; to drink freely of hot drinks; and to take each hour, for several hours, 2 minims of tincture of aconite, followed by ten to fifteen grains of quinia and an opiate if needed. The time during which anything can be accomplished, if such is possible, is lost, as a rule, before the physician arrives.

I have no doubt that some of you think me credulous for my belief in this matter, but I was led to it by reasoning on the method by which I had at times succeeded in preventing the development of those painful paronychias to which I was liable from post-mortem examinations. Moreover, I have seen so many different plans of treatment pursued in cases of pneumonia which ran a short course—three, four, or five days—that I do not wonder at the skepticism conjured up by the name pneumonia. I allude to four cases in one month terminated within four days under different plans.

That the words empyema, appendicitis, typhoid fever, immediately conjure up to different physicians very different mental pictures as to the necessities of treatment, goes to show that there has not been reached a unanimity of belief as to the wisest course to pursue, and that probably in each of these selected instances, instead of treating the morbid condition present in each given case, which of necessity must vary and call for different measures, we are influenced in some by the name and act accordingly.

For some time phthisis was to many physicians the synonym for a disease of a progressive character. But there have come waves of treatment, each promising success, now by sulphurreted hydrogen, then by hot air, and lastly by tuberculin. When the former and the latter came into

vogue, most of us still remember the inquiry, "Have you a case of phthisis for treatment?" These accomplished some good, by greater attention being paid to the nutrition and to the general condition of the patient, as well as by the influence of mind upon mind and matter.

DISCUSSION.

DR. W. H. THOMSON.—I am extremely gratified with the points that have been brought before us by Dr. Janeway, because in nearly every particular and detail I fully coincide with his views, which have been entertained by me for a long time. With reference to the number of drugs in our Materia Medica, my most practical comment is that in my lectures on the subject I usually limit myself to about one eighth of the remedies that are officinal, so that many of them I do not refer to at all. I have often made the statement to my class that while there are forty-four officinal preparations of iron, they would be examined on only about six. I cordially agree with Dr. Janeway that it would be a great gain to the profession if some representative classification of drugs could be made, with the object in view of reducing their number, and relegating to a secondary or tertiary position many of those preparations which we find enumerated in our text-books, and which possess the same properties. I once illustrated this fact by quoting from the "United States Dispensatory" the alleged properties of a certain remedy; it was highly recommended as a tonic, an expectorant, a diaphoretic, an emmenagogue, and a deobstruent (whatever that may be), besides possessing other valuable properties, and that remedy was—horehound.

One complaint that has been made regarding my method of lecturing is that I omit certain drugs about which the competitors for hospital positions are likely to be asked, and this, I regret to say, is true. I feel inclined to criticise those who were concerned in the revision of the last Pharmacopœa for altering the strength of many valuable preparations, such, for instance, as opium. Such changes are apt to lead to serious accidents. Moreover, there are more of these preparations than there is any necessity for, and on this point I think the profession should take a firm

stand. We could get along very well with two, or at most three, preparations of opium. In the new Pharmacopœa, the strength of tincture opii has been increased by 25 per cent. and the officinal strength of the solution of sulphate of morphine is now four times as great as in the last edition. Magendie's solution in this country contains sixteen grains of morphine to the ounce, which is considerably greater than in France, and I have known of one case where a hospital interne who studied in Paris administered hypodermically a dose of this solution which resulted in the death of the patient. Again, let us take aconite. This is one of the most valuable drugs we have, and one which will always hold its place. Furthermore, it is an extremely powerful drug, and why should we have more than one tincture? In addition to that we have the fluid extract, a very dangerous preparation, the dose of which for an adult is only one-half minim. Then we also have the solid extract of aconite. The present preparation is nine times as strong as that which preceded it, which was made from the leaves, and which was so weak that I think it was very proper to change it, still, why was it necessary to make it so much stronger?

As regards the administration of drugs in tablet form I am also fully in accord with Dr. Janeway. For instance, quinine should not be used at all in pill form, unless the pills are very recently made. I remember one case where a man took sixteen gelatine covered quinine pills in the course of a day, followed by a cathartic, and on inspecting the stools he found that fourteen of the pills had passed through unchanged. In some of these tablet preparations, I think, the drugs soon lose their activity. This is so with nitroglycerine, which I always prefer to prescribe in solution.

As regards the practice of treating the names of diseases instead of treating patients, I think we are making decided progress in some directions and losing ground in others. The names of certain diseases have no significance whatever. I should like to know what is the matter with a person who is said to have neurasthenia? We say that we are treating a person for anæmia: why should we not say that we are treating a patient for emaciation? We are not only treating the names of diseases, but we are also treating the disease with names. In a certain case

which I saw in consultation, the physician associated with me objected to giving quinine on the ground that it would be injurious to give a tonic. What is a tonic? The word took its origin from a fiddle-string which gives rise to a certain tone, and now we read that a tonic increases the tone of muscular or nerve fibre, and the term is applied indiscriminately to anything that is supposed to do good. Cod liver oil, baths, electricity, massage,—all these are tonics. We have no real conception what is meant by the term. I would like to see radical changes made in the classification of our medicines. Instead of such vague terms as alteratives, stimulants, sedatives, etc., each remedy should have its properties clearly defined and classified.

With reference to mineral waters which possess no distinct medical properties I fully agree with the author of the paper. We have every reason to warn the public against frauds in this direction.

DR. G. L. PEABODY.—I have been very much interested in Dr. Janeway's paper, and I think we all owe him a debt of gratitude for his introduction of this topic, As a matter of fact, I agree with almost all of his propositions. I have always been disinclined to teach my students all the preparations of the various drugs, and it has seemed to me that our function, as teachers, was rather to lay down certain guiding principles, which will enable the students to know when a certain drug is called for, and what the properties of that drug are. I have never advised them to learn all the preparations of iron, and only expect them to know two or three of the best. This is also true with reference to a long list of other remedies. One thing which has forced me to speak about a greater number of preparations than I otherwise would, is that great bugbear, the hospital examinations. When I began teaching materia medica, nine years ago, I at once decided to make one innovation, and that was, to omit the botanical names of the drugs. Soon afterwards one of the physicians of a prominent hospital came to me and informed me that unless our students knew the botanical names of drugs, they could not get into that hospital. As those appointments were considered rather desirable by the students, I was compelled to teach them the botanical name of each drug, although I did not know it myself. In connection with these hospital examinations I wish to say that in my

opinion many of them are extremely faulty, a fact which is largely due to the fact that they are conducted by men who are not teachers. Some of the questions I have seen could only have been propounded with the view of puzzling the students and showing the wisdom of the examiners.

With reference to the value of ergot I am entirely in accord with the statement made by Dr. Janeway. It is worse than useless when given for the purpose of arresting pulmonary or intra-cranial hemorrhage. It does contract the vessels of the uterus—perhaps also of the intestines—but for hemorrhage elsewhere it is useless. This is also true with reference to a long list of styptics. I fully agree with the statement made that the preparations of opium are entirely too numerous. Laudanum, for instance, which is derived from the crude opium, should have been abolished long ago. The lead and opium wash, which is no longer officinal, I believe, can accomplish nothing more than to stain the linen. We must bear in mind the fact, however, that a remedy which may appear comparatively worthless, to-day, may be found fairly useful to-morrow. Take, for instance, boric acid, which was first described about 1702. It was introduced as a sedative salt, but it was soon discovered that it was not a salt at all. It was retained as a sedative drug until 1847, when a German investigator found that even very large doses taken internally had no constitutional effects whatever. It has since been learned that the drug, while it is harmless in ordinary dose, exerts a certain amount of inhibitory power over the growth of bacteria, and for this purpose it is still largely employed.

With reference to mineral waters, or so-called mineral waters, I think we are all in accord. It is rather painful to see the extent to which these waters are endorsed by certain physicians. Taking the natural lithia waters as an example, there is not one on the face of the globe which contains, in a reasonable volume, a therapeutic dose of lithia.

As regards the use of tablets and ready-made remedies, I think that practice has become entirely too common. Many country practitioners employ these ready-made preparations entirely, and I am sorry to observe that they are giving up their old-fashioned custom of compounding

their own remedies. In some of these pills and tablets the ingredients are apt to be unevenly distributed. In one case coming under my observation, a child was poisoned by a preparation which was supposed to contain a very small dose of Dover's powder, and the only explanation I could find was that the ingredients had been unequally distributed.

DR. B. ROBINSON.—I was unfortunate enough to get here late, and only heard the last part of Dr. Janeway's paper. Still, I was able to obtain a very good idea of the paper from the discussion, and in all the essential points I agree with its author. I certainly agree with the general proposition that there are many officinal preparations put down in our Pharmacopœa and taught to the students which are more or less useless and seldom or never employed by the practitioner. I also agree with the statement that the use of the ready-made pills and tablets is becoming too general. Still, there is another side to this question which must not be overlooked. Many of our patients are intelligent, well-educated people, who have obtained in one way or another more or less knowledge regarding diseases and drugs, and it is difficult to keep such patients from familiarizing themselves with the ordinary prescriptions. This difficulty can be obviated in two ways: one of these, which has been adopted by certain specialists, is to keep a special formula at certain drug stores, and give the patient a prescription filled with hieroglyphics, with instructions to have it filled at one of these stores, where the clerk interprets it correctly. This is done in order to leave the patient in ignorance as regards the contents of the prescription, and also in order to prevent him from passing it around among his friends. Another and simpler way, which I have personally found very convenient, is to carry in your pocket-case a certain number of pills and tablets, upon the virtue of which you can rely.

As regards the hospital examinations, I cannot entirely agree with the remarks made by Dr. Peabody. It is true that there are some hospital examiners who have never taught materia medica, and who trust that they will never be obliged to. The idea in asking these questions is to ascertain whether the graduate is capable of taking care of the patients in the wards, and all my questions have

been asked with that point in view. In considering the value of remedies, we also should bear in mind their palatability: thus, for instance, tincture of ferric chloride is an excellent drug, but there are certain objectionable features connected with its administration, as I can personally testify by the effect it had on my teeth. In Basham's mixture the same remedy is contained in a much more palatable form. Instances of this kind might be multiplied.

As regards the use of mineral waters, while I agree with the previous speakers that many of them are worthless, there are others, especially from certain of the springs in Europe, to which we must attribute a certain amount of value, no matter what a chemical analysis of the water shows. The testimony to this effect is so general that we cannot disregard it.

DR. H. M. BRIGGS.—I think Dr. Janeway has placed before us in admirable form the opinions of almost every teacher of medicine—certainly of materia medica, and we ought to feel under great obligation to him for it. The enormous number of unnecessary preparations contained in our text-books has always been a serious obstacle to the practical teaching of materia medica. I have always been in the habit of saying to my students that of the numerous preparations of certain drugs, they would be asked by me on only a few of the best. In view of the state and hospital examinations, however, it is often necessary to include a certain list of drugs which in my opinion will be of no future service to them.

As regards the practice of treating the name of a disease instead of the patient, to which Dr. Janeway referred, a practice which is unquestionably a very general one, I think the difficulty lies in the fact that in our medical courses the study of pathology is for the most part omitted. A few years ago there was no school in which pathology was taught to any extent. It is impossible for any man to reason out a rational system of therapeutics without having at least a thorough knowledge of the physiology and anatomy of a disease, and its pathology. This defect, I am glad to say, is now being remedied, and one great thing which it aids in accomplishing is the assistance it affords in making students think.

As regards the use of mineral waters, I think that among the laity and even among the profession there is a feeling

that Providence has placed on earth a number of waters which were specially designed to cure certain forms of disease. I do not agree with Dr. Robinson that these waters possess any inherent medicinal virtues independent of their known constituents; I believe we can get just as much benefit from the same amount of water, plus the chemical constituents. At Carlsbad and other European spas the benefit following a course of treatment is due more to the restricted diet and the manner of living, the whole *régime*, than to any peculiar influence of the waters themselves.

DR. B. ROBINSON.—I desire to place myself on record correctly regarding this question of the value of mineral waters. I do not attach the slightest degree of importance to the reports and analyses we daily receive regarding the virtues of certain so-called mineral waters, which are as ephemeral as the paper on which they are written, and at once consigned to the waste-basket. There is another kind of evidence, however, which comes from men of undoubted integrity, to the effect that certain mineral waters have unquestionable efficacy in the treatment of certain forms of disease, and yet the explanation for this cannot be found in a chemical analysis of the waters.

DR. G. L. PEABODY.—The discrepancies of opinion regarding the value of mineral waters may perhaps be explained by the fact that at many of the well-known spas the patients are instructed to sip the waters very slowly. It has been proven that this sipping process itself exercises a certain physiological influence, and directly increases the flow of bile. This may account for the fact that some of these waters are more efficacious when taken at the springs than when they are transported to a distance.

DR. E. G. JANEWAY (closing the discussion).—In my paper I did not attempt to enter into the subject of mineral waters, and simply touched on the abuse of the term, because so many of these waters are advertised as "medicinal waters" which possess no medical qualities at all. Personally, I hold some pretty strong views on the subject of mineral waters, and am rather inclined to agree with the statement made by Dr. Biggs that they are no more efficacious than water from any other source, plus the mineral ingredients. That they are greatly misused I have no doubt.

Stated Meeting, February 21st, 1895. In Charge of the Section on General Surgery.

DISCUSSION ON AMPUTATIONS.

A.—THE MORTALITY FOLLOWING AMPUTATIONS. A STUDY OF SEVEN HUNDRED AND THREE AMPUTATIONS IN EIGHT LARGE HOSPITALS IN THIS CITY.

By JOHN F. ERDMANN, M. D.

Out of the 703 amputations performed in eight large hospitals in this city, by the various men at those institutions, there were over 109 deaths, or a mortality of 15.5 per cent.; these were divided as follows:

	Wrist.	Forearm.	Elbow.	Arm.	Shoulder.	Foot.	Leg.	Knee.	Thigh.	Hip.	Total.
Amputations.....	7	71	6	88	24	64	156	46	223	1.8	703
Deaths.....	0	1	0	16	6	5	19	6	48	8	109
Per cent.....	0	1.4	0	18	25	7.8	12	13	21.5	44.4	15.5

Of these deaths thirty-one, or 29 per cent., were due to shock of the injury; twenty-one, or 20 per cent., were due to septicæmia previous to the amputation; eighteen, or 17 per cent., were due to constitutional causes; twenty-seven, or 26 per cent., were due to operative shock; one, or less than 1 per cent., of all deaths was due to secondary hemorrhage; three, or less than 3 per cent., due to post-operative septicæmia; three, or less than 3 per cent., due

to nephritis and pneumonia; and one, or less than 1 per cent., due to bichlorid poisoning.

Under the heading of freshly traumatic and non-traumatic, there were, freshly traumatic, 275 cases with fifty-nine deaths, or 21.4 per cent.

Non-traumatic, including old traumas requiring amputation, diseases, etc., 433 cases, fifty-one deaths, 11.7 per cent.

In looking over the various records I find one of the hospitals with a mortality of 16 per cent., and owing to its being so near the average in mortality I have taken it as a standard, with a view toward seeing the improvement in the two halves of the time taken, and present to you the following in recent traumatics:

	Cases.	Av. age.	Deaths.	Av. age at death.	Per cent.
1884–1889.....	47	34	12	43	25.5
1889–1894.....	20	35	4	58	20

The death rate of the first half being 25.5 per cent., with the average age at death of forty-three, while the second half gives a death rate of twenty per cent., and at an average age of fifty-eight, giving us an improvement of 5.5 per cent. in the mortality, although an average increase in age of fifteen years.

Taking all cases we have:

	No.	Deaths.	Per cent.
1884–1889.....	114	19	16
1889–1894.....	61	9	14

This gives us an improvement of 2 per cent. in all cases during the five years from 1889 to 1894.

There were 175 cases amputated during these ten years, of which 108 were for disease, etc., with a death rate of 12, or 11 per cent.

Comparing our mortality with that of other hospital records, I quote, "*Dent & Bull*, Lond. *Lancet*, June, 1890, *p.* 1,303: 1874 to 1888, 400 cases, 21 per cent. deaths." A record of fourteen years, ten of which antedate the first year of our records, during which period

antisepsis and asepsis were imperfect as compared to the years of 1884 to 1894.

"*Schede*, in Pitha & Billroth, vol. 2–2., *p.* 239, reports 377 cases, 110 deaths, 29.18 per cent." Of these three died of secondary hemorrhage. This equals 3 per cent. of all deaths as compared to 1 per cent. in our records. It is but fair to say that these occurred previous to 1882.

Fred. Page, Lond. *Lancet*, 1892, *p.* 523. Reports of Royal Inf. New Castle Tyne, "1883–1891, single amputations, 200 traumatic, 20 deaths, 10 per cent.; 320 disease, 15 deaths 4.6 per cent. 1891, 79 cases of his own, 7 died; equals 8.8 per cent."

Gerster, "Antiseptic and Aseptic Surgery," 1890, *p.* 60: Reports 1,000 unselected hospital cases with a mortality of about 10 per cent. Forty-three individual cases with five deaths, 11.6 per cent.

Estes, W. L., *Med. Record* (N. Y.), Nov. 3. 1894: Reports 294 single amputations with but 4.76 per cent. mortality.

The mortality of Dr. Estes's cases being so low I wrote him asking for the average age of his patients, and received an answer to the effect that as near as he could remember twenty-five years was the average. The age in his cases without question is a marked factor in the low mortality rate, but another and more mighty one can be found in reading the following on page 549:

"In the list of single amputations I have not included "amputations done for purely euthanastic purposes in cases "of evulsion complicated with other serious injuries, when "the patients arrived at the hospital in moribund and utterly "hopeless conditions."

In conclusion I would say that the records we present to-night are unselected single amputations; that the records of individual operators are far better than in combined hospital work; that age is a marked factor in the mortality rate.

B—GANGRENE OF FLAPS.

By HOWARD LILIENTHAL, M. D.

Gangrene of an amputation flap is an annoying complication. It often renders other operations necessary, sometimes even reamputation, and it invariably prolongs convalescence. Secondary operation in an individual weakened by previous shock or disease is a serious matter, and in certain diseases, such as diabetes mellitus, or arterial sclerosis, is especially to be dreaded.

Conclusions drawn from statistics like those under discussion should be general, because of unavoidable inaccuracy in grouping cases of a given class. Under "flap gangrene," for example, are all cases whether of extensive sloughing or of marginal necrosis. A series of cases with slight marginal necrosis may be an example of good surgery with asepsis and rapid healing, yet it comes under the heading of "gangrene of flaps."

Those general conclusions which we may draw from our statistics may, however, be of value because of the large amount of material and the presumably uniform mode of wound treatment.

We find that in 367 cases where the flaps consisted of skin alone, 69, or 19 per cent., sloughed in whole or part, while in 208 cases with musculo-cutaneous flaps, 31, or 15 per cent., necrosed. We therefore note an apparent advantage of 4 per cent. for the musculo-cutaneous flap.

In those cases where necrosis was attributed to sepsis, we note the greatest apparent advantage from the employment of skin and muscle. Twenty-six per cent. of the gangrene cases belonging to the skin-flap group were due to sepsis, while but 16 per cent. of the musculo-cutaneous group sloughed because of infection, the skin flap being apparently more easily killed by sepsis than the better vascularized musculo-cutaneous mass.

The gangrene cases where traumatism before operation caused the necrosis, form 36 per cent. of the skin flap list, while 50 per cent. of the skin and muscle flaps sloughed. This comparison may, I think, be entirely discarded in judging the merits of the two methods, because it is very probable that the viability of the mangled tissues was misjudged by the operators.

There were 20 cases of gangrene of skin flaps due, it is supposed, to constitutional diseases such as disturbed nutrition. These 20 cases constitute 29 per cent. of the skin gangrene cases. Ten cases, or 32 per cent. of the gangrened musculo-cutaneous flaps, show, therefore, a pretty even distribution, there being a difference of but 3 per cent., which is, rather oddly, in favor of the skin flaps.

An instance of gangrene, due to tight bandage, also appears among the skin flaps.

The total number of major amputations in three of the eight hospitals here represented, was 325, and there were 42 cases of flap gangrene, 21 below the knee and 21 in all other regions taken together. There were 83 amputations of leg and foot, with 25.3 per cent. gangrene and 242 other amputations with but 8.6 per cent. gangrene.

From this rapid consideration of the gangrene statistics, I believe we may conclude:—

I. That, other things being equal, a skin and muscle flap is less liable to necrose than one made entirely of skin.

II. That in cases where amputation for traumatism is undertaken, correct judgment as to the viability of the tissues is the determining factor in avoiding flap gangrene.

III. That constitutional disease is a frequent cause of flap necrosis.

IV. That in amputations anywhere below the knee the tendency to flap gangrene is most marked, and that here, wherever possible, the musculo-cutaneous flap is the one to be preferred.

C.—SECONDARY SUTURE OF AMPUTATION WOUNDS.

BY FRANZ TOREK, M. D.

Of the 703 cases in our amputation statistics, 37 were closed by secondary suture. They were 17 thighs, 12 legs, 6 arms, 1 shoulder, and 1 forearm; 20 were closed within forty-eight hours, and 17 later. The former cases I have termed, "Early Secondary Suture;" the latter, "Late Secondary Suture."

Results.—The results were as follows:

No death.	
Primary union	17 cases, 46 per cent.
Primary union with sinus	12 cases, 32 per cent.
Granulation	8 cases, 22 per cent.

Seventy-eight per cent. of the cases therefore united, 22 per cent. did not.

If we take the cases closed by early secondary suture separately, we get the following results:

Primary union	11 cases, 55 per cent.
Primary union with sinus	4 cases, 20 per cent.
Granulation	5 cases, 25 per cent.

The cases of late secondary suture give the following results:

Primary union	6 cases, 35 per cent.
Primary union with sinus	8 cases, 47 per cent.
Granulation	3 cases, 18 per cent.

Material.—The material for secondary suture must be non-absorbable and unirritating. Silkworm-gut is prefered by most surgeons.

Method.—The method of applying the suture is the following: The sutures are introduced at the close of the operation, but instead of tying them, a knot simply holds the ends together. All the sutures, and with them the flaps,

are then lifted up, exposing the wound like an open pocket, which is packed loosely with aseptic or antiseptic gauze. The dressing is then completed. The packing is removed and the sutures tied as a rule within forty-eight hours. No drain is introduced except perhaps a small strip of gauze here and there, carried only through the skin, not into the depth of the wound. The suture may, however, be closed later. Even granulating surfaces, if they are aseptic and if they can be properly adapted, will heal together like fresh surfaces. Granulating surfaces that discharge will of course not unite, but aseptic ones will.

Another method of secondary suture recommended by Nussbaum[1] has not been used in any of our cases.

Instead of introducing the sutures at the time of operation, they may of course be introduced when it is desired to close the wound. This method of procedure is especially indicated, when at the time of operation it is evident that the condition of the wound is such as to require open treatment for some time. A particularly interesting case of this nature was the following: Male patient, aged fifty-one, whose thigh was amputated in the upper third for diabetic gangrene of foot and leg. At the line of amputation, although it was a considerable distance above the gangrenous part, the tissues were still infiltrated and a number of small abscesses were exposed in the line of incision; these were cleansed and stuffed separately with strips of iodoform gauze and the whole wound packed, frequent antiseptic irrigations being subsequently applied. The wound was sutured three weeks later and healed by primary union. In this case the granulating surfaces were freshened before being sutured, as it was impossible to adapt them properly without so doing.

When to close the wound.—Although granulating flaps may unite by primary union it seems that the prospects of

[1] Nussbaum. Eine schmerzlose und unblutige Sekundärnaht. München, 1890.

primary union of the flaps are lessened the longer you wait. In our statistics, of the early secondary sutures 55 per cent. healed by primary union; of the later ones, only 35 per cent. Healing with a sinus occurred in 20 per cent. of the cases of early secondary suture, whereas in the cases of later secondary suture we had sinuses in 47 per cent. These figures should certainly influence us to close our wounds as early as possible. The reason why a late secondary suture should not give so good a result as the early suture is not far to seek. Primary union depends mainly upon two factors, proper adaptation of the parts and asepsis. Now if flaps have granulated for some time, they are neither so likely to be aseptic, nor will they be as thoroughly adaptable to each other as fresh flaps.

But in drawing a conclusion from the statistics we must not forget that there were differences in the nature of the cases, for, whereas the early secondary suture was at times a method of election, *i. e.*, was employed when a simple drain might have answered, the later suture was performed, because the stump after forty-eight hours was not found to be in a sufficiently good condition to warrant closure; they were therefore less favorable cases. The statistics simply point out to us that we should elect early secondary suture whenever we can. We will undoubtedly all do so, if the wound is dry, aseptic, not œdematous, and if its circulation is good, and if this is not the case, there is no choice left for us but to postpone suturing till the parts are in the desired condition; but do not let us wait any longer than necessary.

Indications.—I shall conclude now by stating the indications for secondary suture in amputations. Upon this question the literature is exceedingly meagre. I have therefore been compelled to draw my conclusions as regards the indications for secondary *versus* primary suture chiefly from the cases which I have had the opportunity to study, and from my own experience. But as

there are among my hearers a number of surgeons whose experience is much larger than mine, I would request them especially to criticise these indications and to supplement my list, if it is incomplete. Secondary suture should be employed instead of primary suture.

1. Where on account of persistent parenchymatous hemorrhage it is impossible to get a dry wound. This is liable to happen if an Esmarch tourniquet has been in place for a long time. In such cases tamponing for twenty-four hours will usually suffice.

2. In case we fear secondary hemorrhage, for instance in unmanageable or delirious patients who are likely to injure the stump.

3. If we have amputated through œdematous tissue, we should tampon till the œdema has subsided. The flaps can then be more perfectly adapted.

4. When the circulation is questionable on account of atheroma of the vessels, it will be safer to tampon and watch our flaps for some time before uniting them by suture.

5. When in traumatic cases flaps are used whose vitality is questionable owing to the injury they have received in the accident.

6. If the amputation wound is not aseptic. This includes all degrees of local sepsis, severe, mild, and even the wound that is simply not above suspicion. The one case which I have cited above, shows that even cases amputated through tissues infiltrated with pus may get well, if packed and suture deferred till later. Suspicious cases are found frequently in diabetics and still more so where amputation was done for septic processes, and in these cases the secondary suture should probably be used much more frequently than it is.

7. In pyæmic cases, though the chances of recovery are exceedingly small, we shall not render them still worse by closing the wound. In these cases secondary suture, if it can be done at all, will always be applied late.

While the above are the main indications for the use of the secondary suture it may also be employed without any of these indications and has been so employed. H. Schmid[1] recommends tamponing with iodoform gauze even for aseptic wounds as the safer procedure in cases of private and country practice and in war, to be followed eventually by secondary suture. If so employed, the method of secondary suture simply serves as a substitute for the drainage-tube. And it is certainly an excellent substitute for the tube. If we have employed antiseptic lotions, there is always a certain amount of serous exudation which, as it comes from the entire surface of the flaps, will be drained by a loose gauze packing left in place for twenty-four or forty-eight hours better than by a drainage tube.

The ideal method, however, of closing an amputation wound is, and always will be, immediate adaptation of the flaps in some manner without drainage; but where the chances of primary union are in the least degree doubtful, better results will often be obtained by a secondary suture.

D.—DRAINAGE IN AMPUTATIONS.

BY P. R. BOLTON, M. D.

Prompt union under a single dressing is evidently as desirable in wounds made by amputation as in those inflicted in other operations; and indeed, if, as is often stated, the presence of an excess of scar tissue is likely to be the cause of painful stumps, healing with the smallest possible scar becomes essential to the future comfort of the patient, and consequently to the usefulness of the stump.

While drainage is often indispensable, and in many cases enables the surgeon to secure primary union in wounds which would otherwise heal by granulation, it still

[1] Berliner Klinik, 1889.

has disadvantages which of late have led to its omission in the treatment of many wounds formerly drained.

In aseptic cases drains may possibly convey infection to wounds, and their timely removal necessitates a change of dressing which in itself exposes the wound to possible infection. The more prolonged retention of a drain may lead to canalization of its track and consequently to great delay in the final healing.

The only purpose a drain can serve in an aseptic wound is to remove the wound secretion formed in varying quantities and depending upon the size of the wound and its treatment during the operation. By creating the conditions known to be succeeded by a minimum secretion, viz., absolutely hæmostasis, and accurate apposition of wound surfaces by pressure and by suture, special provision for drainage may be made unnecessary.

It is obvious that the antiseptic irrigation of aseptic wounds, formerly in vogue, is useless.

In the case of amputations made through tissues already infected, drainage becomes necessary to afford escape to secretions certainly containing both bacteria and toxines, agents whose presence would otherwise not only prevent union of the wound, but whose absorption gives rise to pyæmia and septicæmia.

Under these circumstances drainage often serves a most useful purpose, and will probably continue to do so until antiseptic surgery, properly so-called, reaches a higher plane of development.

In connection with this class of cases, reference should also be made to a paper read by Crede at the last Congress of German Surgeons, in which he advocates in the treatment of all amputation wounds no drainage and no sutures, stating that very perfect apposition can be effected by the bandage alone, while drainage is not at all interfered with.

But for practical purposes the wound treatment, so far as

drainage is concerned, as deducible from a study of the cases under consideration, may be relied upon.

Of 490 cases treated by suture and drainage, 167 healed by first intention, and 128 by primary union with sinus, while 195 healed by granulation after more or less extensive separation of flaps by accident, or to secure more efficient drainage.

Of these 490 cases, 142 were amputations of the upper extremity; and of these 72½ per cent. healed by primary union with or without sinus; and of this number 37 per cent. healed by primary union without sinus.

Of amputations of shoulder, 41 per cent. healed by first intention without sinus, 39 per cent. with sinus; of arm, 41 per cent. without sinus, and 37 per cent. with sinus; of forearm, 31 per cent. healed without sinus, and 31 per cent. with sinus.

Three hundred and forty-eight of the 490 amputations were of the lower extremity; and of these, 55 per cent. healed by primary union with or without sinus; and of these, 33 per cent. healed without sinus:

Of hip, 30 per cent. without sinus, 40 per cent. with sinus.

Of thigh, 37 per cent. without sinus, 23 per cent. with sinus.

Of knee and leg, 32 per cent. without sinus, 21 per cent. with sinus.

Of foot, 30 per cent. without sinus, 16 per cent. with sinus.

Of 56 cases treated by suture without drainage, 34 healed by first intention, 8 by first intention with sinus, and 14 by granulation. Of the 56 cases, 15 were of the upper extremity; of these 9 healed by primary union, all of the forearm; 2 by primary union with sinus, one of the forearm, the other of the arm; 4 granulated, 2 forearms, one elbow and one arm.

Forty-one of the 56 cases were of the lower extremity;

and of these 25 healed by first intention—12 of the leg, 1 of the knee, 9 of the thigh, and 3 of the hip; 6 healed by primary union with sinus—4 of the hip and 2 of the thigh; 10 granulated, of leg 2, of thigh 5, of hip 3.

The determination of the question of drainage must be decided in most cases by the nature of the disease or injury for which the amputation is performed, and we find that of the cases drained, 41 per cent. were amputated for injury; 12 per cent. were amputated for gangrene; 6 per cent. were amputated for sepsis; 41 per cent. were amputated for tumor, etc.

Of the cases treated without drainage, 32 per cent. were amputated for injury; 7 per cent. were amputated for gangrene; 2 per cent. were amputated for sepsis; 57 per cent. were amputated for tumor, etc.

The study of the wound treatment in these cases, together with the closer analysis of a smaller number, those amputations performed at the New York Hospital, pleads for the restriction of drainage to amputations done for sepsis, whether consequent upon traumatism or dependent upon gangrene, and to those fresh injuries where antiseptics have been so freely used as to involve an excessive secretion, or where the limits of sloughing cannot be accurately defined.

The same cases show that drainage may be dispensed with in amputations done, with care, for tumors, for many flesh injuries, and for most cases of chronic joint disease.

E. STATISTICS OF 182 AMPUTATIONS PERFORMED AT THE ROOSEVELT HOSPITAL DURING THE LAST TEN YEARS.

BY SINCLAIR TOUSEY, M. D.

The cases herewith presented form part of the 576 cases studied by the other gentlemen, we having jointly collected cases from the eight leading hospitals in this city covering the ten years ending December 31, 1893.

The minuteness of the Roosevelt histories and the amount of time I was able to devote to their study, have enabled me, in addition to the general conclusions from all the hospitals, to collate the details given in the following:

STATISTICS FROM ROOSEVELT HOSPITAL.

CASES.	Shoulder.	Arm.	Forearm.	Hip.	Thigh.	Knee.	Leg.	Foot.
181 cases	9	28	15	7	58	16	39	9
32 died	3	7	0	4	11	1	6	1
71 prim. union	3	11	8	1	22	7	14	5
43 prim., with sinus	3	9	2	1	12	4	10	2
Days, with sinus open	38	17	21	90	42	31	35	120
18 necrosis flap	0	2	3	1	5	1	6	0
3 necrosis of bone	0	0	0	0	1	1	1	0
2 sec. suture	0	0	0	0	2	0	0	0
27 healed by gran	0	2	4	1	12	0	7	1
6 reamputated	0	0	1	0	0	3	2	0
145 final result O. K.	6	22	14	3	47	12	33	8
0 neuralgia	0	0	0	0	0	0	0	0
1 conical stump	0	0	0	0	1	0	0	0

CASES.	0 to 10.	10 to 30.	30 to 50.	Over 50.	Inj.	Dis.	Shock.	Hemorrhage.	Skin Flap.	Skin and Muscle.	Packed.	Rubber Drain.	Bone Drain.	0 Drain.	Buried Suture.	0 Buried Suture.	Silk Skin Suture.	Cat-gut Skin Suture.
181 cases	15	88	57	23	104	77	42	10	110	70	9	115	46	10	74	84	119	51
32 died	3	12	10	7	20	12	17	1	28	13	6	17	8	0	11	16	15	12
71 prim. union	6	41	21	4	32	39	10	4	41	32	0	40	24	6	38	32	51	21
43 prim., with sinus	2	22	13	7	29	13	9	4	30	14	1	31	11	1	25	18	31	12
30 days with oper. av'ge	61	33	22	59	29	51	27	25	35	35	21	39	35	20	31	39	35	37
18 nec. flap	9	7	8	4	13	5	2	0	12	6	1	14	0	3	15	1	14	3
3 nec. bone	0	0	3	0	1	0	1	1	2	1	0	1	2	0	1	2	1	2
2 sec. suture	0	1	1	0	2	0	1	0	2	0	0	1	0	0	1	0	1	0
27 heal. by gran	3	12	9	5	17	10	6	1	22	8	1	24	1	4	18	8	20	7
6 reamputated	1	2	1	2	2	4	0	0	4	2	0	3	2	1	2	4	3	3
145 fin. result O. K	11	75	44	16	82	63	25	9	90	56	3	94	36	10	81	57	102	38
0 neuralgia	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
1 conical stump	0	1	0	0	1	0	0	0	1	0	0	0	1	0	0	1	0	1

CIRCUMSTANCES AFFECTING MORTALITY.

(181 cases, 32 deaths, 18 per cent.)

1. *Age.*—(Excluding the forearm where the mortality was zero and where the proportion of patients over fifty years old was relatively much greater than elsewhere). After fifty the prognosis becomes very much more grave, while under ten the prognosis is about the same as from ten to fifty.

Excluding in nineteen cases over 50 years, deaths 7, or 36 per cent.

Forearm in fifteen cases under 10 years, deaths 3, or 20 per cent.

In 131 cases between 10 and 50 years, deaths 23, or 17 per cent.

2. *Site.*—The mortality was greatest in the hip, numbering seven cases, with deaths 4, or 57 per cent. The least mortality was in the forearm. (Sixteen cases, no deaths; thigh, 19 per cent.; knee, 6 per cent.; leg, 15 per cent.; foot, 10 per cent.; shoulder, 33 per cent.; arm, 19 per cent.)

It is interesting to note that of the three cases recovering after amputation at the hip, two were done with Wyeth's pins.

3. *Cause.*—Amputations performed for injury are slightly more fatal than those for disease, the mortality being 19 and 16 per cent. respectively. (Twenty died in 104 cases for injury, and 12 in 78 cases for disease.)

Amputations for gangrene are no more fatal than others, the mortality being 18 per cent. (Three died in 17 cases.)

Amputation for sepsis (cellulitis, septicæmia, pyæmia, infected compound fractures, and the like) are extremely fatal, the mortality being 70 per cent. (Seven died in 10 cases.)

4. *Shock.*—In amputations for traumatism the presence of that profound depression of the nervous system which

we call shock, makes the mortality many times as great as when the symptom does not follow the original injury. In the traumatic cases, in which there was marked shock previous to the operation, 40 per cent. died (seventeen in forty-two); whereas in traumatic cases without shock, the mortality is only 5 per cent. (three in sixty-two cases). The idea will doubtless occur to many that this is merely another way of saying that the more severe the injury for which the amputation is done, the more grave will be the prognosis; for example, all three amputations at the hip for injury died, while three of the four done for disease survived. But this is not the whole truth, for one patient, after a crushing injury, will be in a state of collapse, and will die from the operation, while another, with a similar injury of the corresponding portion, will be found suffering from little or no shock, and will stand the operation perfectly well. The following seems to me to be a fair way to make the comparison: In thirty-four traumatic cases, including only the arm, shoulder, leg, and thigh, in which marked shock was present from the start, thirteen died, or 38 per cent.; in forty-three similar cases without marked shock from the injury, there were only two deaths, or 5 per cent.

The inference from this is that it is the shock from the injury which kills the patient, that one patient is more susceptible to shock than another, and that the operation adds little to the danger.

The advantage of treating shock for more than a few hours before proceeding to amputation is doubtful. I believe that the fact that in the arm, shoulder, leg, and thigh, in six cases in which shock was treated for more than twelve hours, four died, against seven in thirty-two cases of shock treated less than twelve hours, goes to show that the patient has a better chance if the operation is done in the first three or four hours while the nerves of the limb are still benumbed by the injury, than if they are allowed

by long delay to regain their tone, and thus make the operation itself a source of additional shock to the central nervous system.

Multiple Injuries.—These add considerably to the danger, but not, perhaps, as much as one would expect. In my list there are three cases of multiple amputation, only one of which, arm and thigh, resulted fatally. The two which recovered were foot and opposite thigh and both forearms.

SINUS—CIRCUMSTANCES AFFECTING ITS FREQUENCY AND DURATION.

1. *Age.*—It is more frequent in those over fifty years of age (37 per cent.), and least frequent in those under ten (20 per cent.); in those between ten and fifty it occurs in 23 per cent.

2. *Site.*—It is most frequent at the shoulder (50 per cent.), and least frequent in the forearm (13 per cent.); in the lower extremity it varies from 25 to 33 per cent.; in the arm it is 41 per cent. *The average duration of a sinus after an amputation in the upper extremity varies from seventeen to twenty days; whereas in the foot the average is one hundred and twenty days; in the hip, ninety days, and in the leg, knee, and thigh, from thirty to forty days.*

3. *Cause.*—It is present in 29 per cent. of the traumatic cases, and in only 19 per cent. of those performed for tumor or disease.

4. *Wound Treatment.*—Whether a bone or a rubber drainage tube is used, makes comparatively little difference, though the advantage is with the rubber sinus in only 26 per cent., against 31 per cent. with bone tubes.

DISCUSSION.

DR. C. MCBURNEY.—This subject is such a vast one that I shall only refer to a few of the points which have been brought up. Although I have not the figures at hand, I think one might properly claim that the mortality percentage of amputations to-day is lower than it was during the period embraced by the statistics that have been quoted. I make this statement on the ground that the traumatic cases selected for amputation to-day are mostly of a very severe character, and many cases that would formerly have been subjected to amputation, are now, through better methods, saved. That is to say, the modern mortality in traumatic cases is small and yet the cases treated by amputation are all varied and dangerous cases. As regards gangrene of the flaps, I have seldom seen this complication occur. Among its more frequent causes might be mentioned injury to the flaps, interfering with the local blood supply, or it may be the result of a general atheromatous condition of the arteries. I think the local cause is often produced by the operator, and this is borne out by the statistics, which show that skin-flaps necrose more frequently than those composed of both skin and muscle. In cases where the skin-flap is made use of, extreme care should be taken not to injure the subcutaneous tissue, which carries the vessels. The other cause of gangrene, namely, the atheromatous condition of the arteries, or the occlusion of vessels near the point of amputation, may be guarded against to a considerable extent by ascertaining beforehand, as accurately as possible, the condition of the circulation in that region. By making a transverse incision it is easy to learn whether the circulation is good or not.

I am heartily in accord with the remarks that have been made regarding the secondary suture, which I look upon as an important factor in the reduction of the mortality rate. Secondary suture, I think, properly belongs under the heading of drainage; that is what it really amounts to. I do not mean that the method should be applied to perfectly clean wounds; such wounds I think can be safely closed immediately, although personally I always employ some form of drainage. The objections that have been raised against drainage can only apply to those cases where it is

left in for too long a time. In clean wounds, no drainage is required after the first twenty-four, or at the longest forty-eight, hours.

As regards shock, this is a broad, general term, rather vague, but of great interest to us all. Much difference of opinion exists regarding the management of cases suffering from shock, both as to the time of operation and the means which should be adopted to overcome the shock. From my experience, I have no reason to think that under ordinary circumstances the presence of an injured limb for a few hours exerts any additional injurious effect on the patient whatever. I have never believed in the idea that the presence of a contused or lacerated limb either aggravated or prolonged the condition of shock, and therefore I can see no particular object in trying to get rid of the injured member if the patient is not fit for operation. If the limb is loosely attached by shreds it can be trimmed off at the earliest opportunity, all bleeding vessels tied and the wound thoroughly cleansed and stuffed with gauze; then, instead of doing a formal amputation, the patient can be allowed to go on for an indefinite period, until the shock has been recovered from. At a later period a secondary amputation can be done if it then seems to be necessary. In this connection I wish to express the opinion that many cases of shock, so-called, are really cases of hemorrhage, and active means should be employed to overcome it. For this purpose the introduction of salt solution into the veins has given excellent results in my hands. The injections can be made either before the operation, or during it, or immediately afterwards, as the condition seems to require. I usually employ about a quart or a quart and one-half, the solution being at a temperature of from 116° to 120° F.

Dr. L. A. Stimson.—As regards the mortality following amputation, it is very difficult to obtain from the statistics that have been given a fair idea as to its causes, or of the intrinsic dangers of the operation itself, because of the constantly varying elements which unite in producing the results. I was surprised to hear the statement made that the mortality rate of the traumatic and pathological cases was about the same. I think it would have given a more exact idea of the operative risk if the latter group had been divided into two; one containing amputations done for chronic conditions, such as tuberculosis or tumor; the

other containing those done as a last effort to save life threatened by sepsis. The first of these two classes gives a low mortality, the second a high one.

So far as shock is concerned I was surprised to hear the statistics which indicate that a delay of twelve hours or more was followed by a notable increase in the mortality rate, and I asked myself whether there was not some other reason to account for this. Whether it was not due to the fact that in those cases the shock was particularly severe, and the surgeon waited, trying to make up his mind, and finally operating as a last resort to save the patient's life. I was taught that an amputation should not be done during the condition of grave shock, and that the temperature was usually a fairly safe guide to indicate the proper time for interference. So long as the temperature is sinking or remains low, an operation should not be undertaken, but when it begins to rise and approaches the normal, then we could amputate with comparative safety, so far as shock is concerned.

Among the many valuable surgical modifications and procedures which we owe to Dr. McBurney, I think the intra-venous saline injections at the time of operation is one of the most important. I am satisfied that the lives of some of my patients have been saved by this.

In order to avoid necrosis of the flaps in traumatic cases, I have for some years past followed a very simple rule. When the Esmarch bandage is applied to a region where the circulation is good, and left in place for a time, the limb blushes brightly on its removal; while when it is applied to a region where the skin has been so injured that it will slough, that portion of the limb remains white; the outlines of this whitened area of skin serve as my guide in choosing the site of amputation. Marginal necrosis is largely a matter of detail,—it may be due to a tight stitch, or to some damage done to the subcutaneous layer.

As for drainage I think that loose suturing and snug bandaging will give us all we desire in most cases. I do not like to see an amputation wound tightly sutured. I do not use drainage, but substitute for it rather loose suturing along a portion of the line, placing the stitches from three-quarters to an inch apart. The dressing is left undisturbed for ten days, or two weeks, or even longer, when

the wound is practically healed. I am speaking now of aseptic wounds. The first discharges soak into the dressing immediately around the wound, which on drying become fixed and hard, and completely immobilize the flaps, a condition most favorable to rapid healing.

DR. R. ABBE.—I fully coincide with the views held by the previous speakers, particularly with the statement made by Dr. McBurney that the musculo-cutaneous flap is preferable to one composed of skin only. I think that skin flaps often slough because of the compression they are exposed to over the end of the stump by the bandages forming in creases which interfere with the circulation of the parts. Where it is possible, I have made it a rule to use the muscle and skin flap in one complete mass. Another point of importance is the deep buried suture of the muscle over the end of the bone: although of course the muscle atrophies, it leaves a mass of dense tissue which is of service in hiding the bone and forming a valuable cicatrix. With regard to the secondary suture, I think it is desirable in certain cases, where we are suspicious of the integrity of the tissues, to pack the wound open and subsequently approximate by suture or straps. In connection with drainage I am surprised that so much has been said about a sinus remaining. A temporary sinus usually remains when the tube is removed, but this is of no importance. I regard drainage as very essential and it may often save us the trouble of re-opening a wound. Flap gangrene, as the statistics show, is usually most common after foot amputations, and this is probably due to the fact that the bone lies directly underneath the skin, and there is little or no cellular structure to aid in the nutrition of the flaps. For this reason amputation of the middle of the foot is not of much use, because you are apt to get flap sloughing and tender scars which usually give rise to trouble. For a workingman, the best amputation is in the lower third of the leg or thigh, because to a stump in these regions a serviceable artificial limb can be readily adjusted.

I agree with Dr. McBurney that one of the most important factors in the production of shock is hemorrhage, and this should be at once combated. In cases where the shock is severe, I believe that to delay amputation is in accordance with the best rules of surgery.

DR. F. H. MARKOE.—This question has been so thoroughly discussed that it is only necessary to add a few words as regards personal experience. Cases requiring amputation may be properly divided into the traumatic and the pathological. The latter again may be divided into the septic and the non-septic. In those of the last class it has been my rule that, the amputation being done through healthy tissue, the wound should be closed. For some years past I have given up drainage entirely in such cases, using buried sutures and accurately approximating the skin, and I have had no reason to regret it. I agree with Dr. McBurney, that by leaving the injured limb in place, we do not aggravate the condition of the patient. For the past two years my custom in traumatic cases has been to carefully cleanse the injured parts, cut away all loose tissues, then pack the wound and wait for the patient to recover from the shock. I think by this method we are often able to save a good portion of the limb and sometimes the entire limb. The saline injections are often of the greatest service in these cases. Shock is made up not only of hemorrhage, but also of a certain mental condition produced by the injury itself. If we can overcome the loss of blood and sooth the nervous symptoms, the patient's chances are much improved. When the operation is through diseased tissue, tamponing and secondary sutures cover the ground.

DR. F. W. MURRAY.—I am especially interested in that part of the discussion which referred to the mortality rate. Since the introduction of antiseptic surgery the mortality following amputation has steadily decreased, and with the use of our present aseptic methods and with the improvements in hospital hygiene, we have every reason to expect that it will be lower. Personally, I am in the habit of employing drainage in the treatment of amputation wounds. Strips of sterilized gauze or of sterilized iodoform gauze and occasionally strands of catgut are inserted in the angles of the wound and are removed twenty-four or forty-eight hours after operation. In several instances I have employed tamponade and secondary suture with very satisfactory results. In cases where time is an important element, the duration of operation is shortened by using the tamponade, and the dangers of prolonged anæsthesia and shock lessened. By means of the saline infu-

sion we are enabled to cope successfully with shock, and many cases which formerly were considered hopeless, are saved.

Dr. B. F. Curtis.—From the discussion, this evening, it appears that surgeons in New York do not appreciate the advantage of getting rid of drainage as much as, in my opinion, we should. While a sinus left in the wound is, perhaps, not a serious thing, it is annoying to both patient and physician, and prolongs the treatment. Another point is that by doing away with drainage tubes, we get rid of one possible source of infection. As regards gangrene of the flaps, I think that one of its principal causes, which was suggested to me by the President (Dr. Bryant), is that the flaps are cut too short and then tension is made in suturing them over the end of the bone. I agree with Dr. McBurney that during the past five years, as shown by Dr. Erdmann's figures, there has been an improvement in the mortality rate following amputation, in view of the fact that we are now operating in cases which would have been condemned as inoperable some years ago.

Stated Meeting, March 7th, 1895.

MALNUTRITION, PRACTICALLY AND PHYSIOLOGICALLY CONSIDERED.

A SUBJECT OF GREAT IMPORTANCE TO THE SPECIALIST AS WELL AS TO THE GENERAL PRACTITIONER.

By J. HARVIE DEW, M. D.

Malnutrition cannot be classified as a distinct disease and yet in its relative importance, as a subject of study, it should have a well recognized place in the literature of medicine.

Correctly defined, it is an incidental symptom, or rather a condition, but always one to be considered with the greatest care in the scientific management and treatment of disease.

The energy producing and spending capacity of all animal bodies depends directly upon the activity with which the processes of nutrition are conducted and maintained. The continued accomplishment of this essential requisite makes it necessary that a large number of organs and functions shall be in good, if not perfect, physiological order.

1. There must be a comfortable condition of mind and body, and a relish or appetite for food.

2. Mastication must be complete, and the stomach capable of receiving and holding food.

3. The digestive fluids must be sufficient in quantity and up to standard in quality.

4. The absorbtive capacity of the blood vessels and lymphatics along the alimentary canal must be active and effective.

5. The peristaltic and eliminative work of the bowels must be normal.

6. The heart's action must be sufficient in force and regular.

7. The blood must be suitable in quality and in oxygen carrying capacity.

8. The processes of tissue assimilation must be physiologically correct.

9. The elimination of waste matters through the excretory organs must be properly and effectively accomplished.

If we consider for a moment this large and formidable array of functions and processes necessary to perfect our normal nutrition, it will be easy to understand that diseases, indiscretions, imprudent indulgences, and exposures of any and every kind, may readily serve to upset or otherwise disturb some one or more of them, and thus lead in the end to malnutrition.

The intimate relationship of the nervous system to the various organs and functions of the body, serving as it does to unite and harmonize the one with the other, accounts for the fact that organic diseases, of any gravity whatever, are sure to disturb a number of these nutritive functions. Especially is this the case when the febrile centres become excited and the elevation of temperature reaches a high degree. It is an old proverb worth quoting, that "When fever comes in, appetite gets out." As a rule, nothing can be truer.

When illness or disturbed functional conditions have continued for any length of time, the active symptoms of malnutrition exhibit themselves in the loss of appetite, defective digestion, abnormal bowel action, paleness or anæmia, weakness and languor or lack of energy production, feebleness or depression of heart's action, nervous disturbances, loss of weight, emaciation, etc.

In the treatment of acute progressive diseases, in con-

junction with an effort on the part of the physician to combat, or to facilitate, as the nature of the case may require, the active pathological changes which are taking place, he endeavors by every possible means to maintain the nutrition of his patient. This he does through the agency of the most nourishing and at the same time the most easily digestible food at his disposal, stimulants, etc.

When disease becomes quiescent, or is at an end, the work of "building up" or rather of repairing the incidental damages begins.

This consists largely in correcting the functional disturbances and malnutrition which have been induced. In so doing, it is a recognized truth, that in greatly the majority of cases, this affords the promptest method of healing or restoring to a normal condition any pathological lesion which may be found to exist.

Here I desire specially to impress the fact, that in general medicine, there are but few well established specifics. In surgery, the knife may be regarded in many instances as a specific remedy, and certainly as one of inestimable value for the removal, when necessary, of diseased parts. The practitioner and the specialist, independent of the knife, find many local remedies almost specific in their action, as the gynæcologist, the eye and ear, nose and throat, skin and other specialists will verify. But whatever may be their capacity for treating and relieving certain pathological conditions, it must be conceded that the removal of any evidence of malnutrition and the improvement of the general health of the patient play a most important, and often the essential, part in effecting a complete restoration.

This is best accomplished by looking over the various functions and processes necessary to nutrition and getting them into good working order. Then by means of tonic, suitable food, fresh air, etc., the patient secures his best possible chance for perfect recovery.

It is interesting to note the fact, that when by proper attention the physician has successfully overcome the symptoms of malnutrition, his patient will be heard to say months, or even years, afterwards, that he has never been so well as since that spell of illness he had, which he really thinks has proven most beneficial to him. On the other hand, when proper attention is not given to a complete reparation of the damages, nature struggles unaided up to a certain point, stops its efforts, repair is incomplete, and the patient will be heard to say,—"Well, I have never been right since that spell of sickness I had last spring," or whenever it was. This shows the necessity in all cases of restoring the nutritive functions, if possible, to their normal condition, before pronouncing the patient cured.

In the treatment of malnutrition many physicians declare medicines of no value, and trust entirely to good food, exercise, fresh air, mild stimulants, wines, etc., and change of climate.

These are all admirable aids to treatment and in many cases may be sufficient in themselves to accomplish complete restoration, where the means and leisure of the patients will enable them fully and intelligently to carry out the plan, but vastly the majority of cases will be greatly benefited and hastened in their recovery by judicious medication.

As stated, malnutrition has, as its exciting and primary cause, disease and functional disturbances of every kind; hence, scientific rules at once suggest the removal, as far as possible, of any original cause, and if not removable its treatment in accordance with the pathological conditions presented. While this is being done, every attention must be given to the disturbed functions which lead to a loss of appetite and a consequent inability to receive and dispose of a requisite supply of nutritive food, to restore vigor and tone to the system.

No fever being present, the first point to consider is the

condition of the alimentary canal and the secretions which are necessary to the performance of its functions. The bowels should be made free from objectionable residues, and kept clean by gentle purgatives or salines as indications may suggest. Often the judicious use of mercurials without or in combination with other purgatives, especially rhubarb, is of the greatest value in exciting the biliary secretions and giving preliminary tone to the canal.

Among the newer remedies, cascara sagrada is of unquestioned value, and is greatly enhanced in its utility, when frequent administration is required, by its combination with some active diastatic preparation such as malt or one of its modifications.

Whatever loss of appetite may appear to have as its original cause, it will often be found immediately due to an insufficient flow of the digestive fluids, which, as a rule, leads to a coated tongue and to digestive disturbances. In these cases, especially when no fever is present and the active processes of acute disease are at an end, are the bitter tonics indicated and found to be of the greatest value. They excite secretion by direct local contact and should be taken immediately before eating.

Catarrhal conditions of the stomach and upper portion of the intestinal canal leading to the same results, are a common consequence of imperfect digestion during illness, over-feeding and of alcoholic abuses. Hydrastis (the fluid extract) and its preparations are admirable and almost specific in their action.

Hot water, properly administered, is also decidedly beneficial. In many instances where the introduction of food leads to gastric discomfort, if not to pain, most excellent results may be obtained from the use of an infusion of hops (an ounce with two drams of ginger-root to the pint of boiling water) a small wine glassful immediately before feeding.

Continued defective nutrition from almost any cause,

especially from febrile disturbances, tends directly to interfere with the blood-making processes and to establish anæmia.

As soon as the red blood globules become altered in quality and in quantity, the oxygen carrying capacity of the blood is diminished in the same ratio. The digestive fluids are impaired in quality and disturbances take place from that cause. The processes of endosmose and exosmose also depend largely upon the condition of the blood, and absorption becomes incomplete.

All the chemical changes which depend upon a requisite supply of oxygen for their accomplishment, secretion, assimilation, tissue oxidation, elimination, etc., etc., are rendered abnormal.

The establishment of these conditions soon leads to loss of appetite, indigestion, languor, weakness, nervous disturbances, and one or more of many other unpleasant symptoms indicative of malnutrition. Hence, to recognize anæmia in whatever phase or degree it may present itself, and to correct its evil influences, is most important. In many instances a simple improvement of the appetite and of the digestive functions, together with fresh air, etc., is quite sufficient, but it is often found that the deficiency in hæmoglobin, the oxygen carrying element of the red blood globules, cannot be restored by ordinary or natural methods, and additional medication becomes necessary.

Fortunately iron which enters so largely into the composition of hæmoglobin provides, as a rule, a positive remedy for this difficulty when favored by proper hygienic surroundings. Though the preparations of iron are almost innumerable, I cannot say that there is much choice between many of them. Any fairly good preparation will usually answer the purpose. If the appetite is defective or absent, the combination of iron with some one of the bitter tonics, often adds greatly to its efficiency, and should be so prescribed.

When digestion and absorption from anæmia or other causes have become impaired, experience teaches us the importance of introducing the most digestible food possible, and when the quality of the fluids has been altered to any extent, whether during or following upon illness, it is advisable, if not necessary, to administer partly digested foods, such as peptogenized milk, koumyss, matzoon, malted milk food, beef peptones, essences, extracts, etc.

When appetite has been partially or completely restored and discomfort follows the introduction of ordinary food it is a matter of special importance, in order to treat intelligently, that we discriminate with some degree of accuracy between the many conditions which lead to this disturbance and which are commonly recognized as evidences of indigestion or dyspepsia. The most frequent causes are catarrhs of the stomach and upper intestines, neuroses, and deficiencies in quantity or quality of the digestive fluids. Catarrhal conditions have already been spoken of.

Neuroses must be treated in accordance with the indications of each special case: deficiencies in quantity, as stated, by bitter tonics and an increased fluid diet.

Deficiencies in quality have a wide range of causes, effects, and means of relief, consequently they cannot be properly considered within the scope of this paper. Yet, in a general way, I should like to say, that each one of the digestive fluids, when disease or malnutrition has reached a sufficiently advanced stage to bring about the result, is liable to have some one or more of its essential ingredients so altered in character and efficiency as to render the fluid incapable of properly performing its normal digestive functions. In such cases we cannot analyse these fluids, hence are forced simply to consider the symptoms of each individual patient from a practical and physiological standpoint.

Recognizing, as it is best we should, the old and yet

unimproved classification of food elements under the heads of

1. Inorganic matters, water, salts, etc., requiring no digestion;

2. Albuminoid substances, meats, albumen, casein, and such like, which must be properly converted into peptones; and

3. The hydrocarbons, starch and sugar for conversion into glucose, and fats for emulsification, enables us, by closely observing the effects or disturbances produced by each class when introduced. to determine quite accurately the cause of the defective digestion. The distress or discomfort afforded by errors occurring in the conversion of either the albuminoids or the hydrocarbons, as a rule, differs sufficiently the one from the other for diagnostic purposes. If on the other hand the case seems obscure, it will be found easy by adopting a single food diet, or else by excluding one article after another from the usual diet list, to ascertain the cause. When fully determined there will be no difficulty in selecting from among, I might almost say, the ten thousand preparations of more or less value, that now deluge the modern drug-shops, some suitable one to facilitate or aid in the digestion of the one, or the other class, while efforts are being made in a scientific way to alter and improve the general system, and thus to correct the error in secretion.

That pepsin and its combinations, papain, papoid, diastase, pancreatin, extract of pancreas, acids, alkalies, and many other digestives which are being highly advocated, appropriately and judiciously administered, have all a special range of usefulness, there can be no doubt, but some judgment must be exercised in order to make a proper selection and thus to secure the desired benefits. While these ferments or digestives are more direct, and possibly more specific, in their actions, there are other most important aids to the processes of digestion which are very

universally, though often very indiscreetly, taken, as well as prescribed. I refer to alcoholics, which in almost any form judiciously mingled with the food during a meal, are of value, serving in its speedy absorption and combustion, and in its stimulative effects, the same relationship to food and its digestion, that the more rapid combustion of paper and kindling wood holds to coal in starting a fire. Wines of one sort or another and even the stronger liquors are frequently prescribed in suitable cases with most excellent results.

The malt preparations are also of great value, especially those containing a bitter element, not too much alcohol, and a fair amount of nutritive matters.

Black coffee after dinner has its beneficial action as a consequence of its bitter properties, which aids in exciting secretion; and of the caffeine it contains, which provides a desirable cardiac and nerve stimulant.

In correcting many of the unpleasant features of malnutrition, it will be found that a prescription containing some malt preparation, abundant in nutritive and diastatic properties, with an agreeable wine and some pleasant sedative to the nervous system (which is so often disturbed), will prove a decided aid to treatment, administered after meals.

In the malnutrition of wasting diseases the suggestions already offered are largely applicable, but the addition of easily digestible hydrocarbons, such as cod-liver oil and its preparations, is often of the greatest value. They at least provide a very available and easily combustible food, which in many instances serves to lessen, if not to stay, the processes of tissue consumption.

There is still another type of malnutrition with which we frequently meet and have to contend, which is interesting in the fact that it is not the outcome of any special or well defined disease or disturbance, but rather of habits and neglects, often dating back many years, if not to child-

hood. I refer to that form which is exhibited in those with whom we meet in every-day life, and who present a thin, rather emaciated, and badly nourished personal appearance. Many of these cases pass through years and even a life-time without disease or serious discomfort, but when we are consulted by them, or have an opportunity to investigate their general health and habits, we usually find many excellent reasons why their nutritive conditions have been defective.

Vastly the majority of these cases are due to a failure on their part to take a sufficient quantity of fluid nutriment to supply the physiological needs of the body; they are not aware of the fact, and the physicians often forget, that water enters into the composition of the body to the extent of from two thirds to three fourths of its entire weight; that digestion, absorption, the circulation, tissue interchange, and elimination, all require an abundant supply of fluids, and that to increase the healthy weight of the body not a pound of flesh can be formed and added without the utilization of two thirds of a pound or more of water in its manufacture.

These cases will, as a rule, be found to suffer more or less from dyspeptic symptoms and from constipation. Neuralgia, headaches, nervousness, and other disturbances are also frequent accompaniments. Many of them are large and often ravenous feeders, and the wonder of the non-professional observer is, where do they store away so much food, and what becomes of it, as the eater still continues thin and scrawny. The simple explanation is, that the greater part of the solids taken passes through the intestinal canal as undigested and unabsorbed material, and is finally emptied into the water-closet.

It has been my experience that a great deal can be accomplished for this class of patients. Digestion and absorption can be improved, the diet and methods of feeding regulated, and the supply of fluids so increased as to

greatly facilitate the processes of secretion, assimilation, and elimination.

When on the other hand the appetite is poor and a meager or insufficient quantity of food is being taken, any organic or functional irregularities should be closely looked after and corrected. Tonics and digestives are then administered, and when the appetite becomes fair and digestion improved, I usually direct the patient to eat and drink as liberally as he likes at his regular meal, and one hour after to take a supplementary meal consisting of nearly or about a pint of some liquid nutriment. Rich milk is preferred, as I believe it to be the most fattening of all fluids, when it agrees well with the patient. This provides three extra pints of fluid per day and is taken in a way not to interfere with the appetite for the coming meal.

When pure milk does not agree or cannot be taken, peptogenized milk, koumyss, matzoon, butter-milk, beef-tea, chicken-soup, gruel, or some other nutritive liquid should be substituted. Under this treatment, modified as indications may suggest, many pounds may be added to the weight of a majority of these cases, if faithfully kept up for a sufficient time.

The advancement which modern knowledge and science have made in practical chemistry, especially as exhibited in food products, and in our ability to aid and strengthen the digestive processes, has proven of inestimable advantage to the medical man, and has probably done more to facilitate his efforts in combating the evil effects of malnutrition than in any other department of medicine.

It is of course impossible for one paper, or even for one man, to present more than a small portion of what there is to be written upon this broad and almost inexhaustible subject of malnutrition, but if this article will serve to suggest an additional thought to some one of my hearers, or will induce the presentation of still more valuable ideas from some other source, I shall feel highly repaid for what I have said.

DISCUSSION.

DR. A. JACOBI.—I do not think that I ever arose with more misgiving to open a discussion or to participate in the discussion of a paper than I do to-night. Dr. Dew has gone over the whole subject of physiology and pathology, and a good deal of materia medica, and it appears to me that very little remains to be said. I hardly think there is anyone present who has seen man in health and disease but must fully agree with the statements we have heard. The author of the paper is certainly correct in attributing all the conditions of what has been called malnutrition to some derangement of function of one of the many organs, including the blood. As the condition of malnutrition is the result of every disease, its treatment is really a part of general therapeutics, and in order to discuss it fully it would be necessary to consider a great many topics which I do not feel able to take up to-night. Dr. Dew himself said that he could only review the subject, and it would certainly be highly improper for me to try to enlarge very much upon his statement of facts. Still, a few additional points might be added.

It is a peculiar fact that although we have at our command the best so-called nutriments in the world, the cases we have to deal with are so individual in their nature that what is good for one may not be good for another. For instance, we are all ready to admit that milk is the best nutriment for the young and old, particularly for the young. On the other hand, we know that if we give milk indiscriminately to every child that comes along in the condition of malnutrition, we are just as apt to make grave mistakes as not. We know that a child at the mother's breast will not always thrive,—becomes rickety, perhaps,—and that if it is transferred to another breast it may do better. Some babies will not thrive at any breast, and will do better on some sort of artificial food. That is simply because there cannot possibly be a uniformity in the condition of the digestive organs and the secretions. We consider milk the best nutriment, and yet I am not at all ashamed to say that cows' milk has done just as much harm as good. For instance, to put a very young baby on an indiscriminate cows' milk diet is the very worst thing we can do for it. There are just as many babies harmed by

cows' milk feeding as are benefited by it. It is a fact that many cases of rachitis owe their origin to an indiscriminate and exclusive cows' milk diet. If such a baby is fed on modified cows' milk, mixed, for instance, with cereal decoctions, it will thrive; otherwise not. We must individualize in each case, and try to select the best diet. This is only one of the many examples that can be given to show that positive rules cannot be laid down for any special form of treatment. Even when we remove what is considered the dangerous element of cows' milk by Pasteurizing or sterilizing it, the same results may follow. Of late we have been led to believe that when cows' milk is sterilized, it is not only harmless, but is very often the best food that can be given. That is a mistake, as every man knows who has dealt with many children and with sterilized milk, and does not take his indications for treatment from farmers, chemists, and bacteriologists.

Let us pass on to another instance: Iron is usually given as a means of restoring nutrition, improving the condition of the blood, etc. Nothing is more erroneous than to accept the view that iron is the proper thing to give in every case of anæmia or malnutrition. Anæmia may be the result of gastric disturbance, and in such cases iron is not digested at all. As long as the stomach is out of order, iron should not be given. It should not be given when a simple catarrhal or inflammatory fever is present. In many instances arsenic is a better nutriment than iron, but again we must individualize, because there are many who cannot take a drop of arsenic without producing pruritus or erythema, and derangement of the digestive tract. We are all convinced of the utility of cod-liver oil, yet we are equally convinced of the fact that many persons cannot take cod-liver oil at all. In fact, no one substance can be recommended as a remedy for the condition of malnutrition, and Dr. Dew was certainly right when he stated at the very outset of his paper that malnutrition must be considered as a symptom or complex of symptoms, and is the result of some change in the physiological condition of one or more of the organs. We have malnutrition depending on brain affections; in such cases you would probably avoid the use of alcohol, coffee, and caffein. Again, you would avoid alcohol in acute and sub-acute and even chronic cases of nephritis. On the other hand,

in some cases of malnutrition, alcohol would be the proper thing to give. So we might go on, *ad infinitum.*

In conclusion, I would say that I entirely approve of Dr. Dew's position in regard to this subject, and that each case of malnutrition must be considered on its own merits.

DR. W. H. THOMSON.—When my friend, Dr. Dew, asked me to speak on this subject, I told him it was as large as all out-doors, and that I would confine myself to one special department of it. I think that every therapeutist should start out with the principle that he is to treat the patient instead of the disease. I shall confine myself to that form of malnutrition which leads to emaciation. Although the causes of emaciation are very numerous, the more common ones do not number more than six or seven; the others are so extremely exceptional that for practical purposes they can be left out of consideration.

The first and most common cause of emaciation is fever. Many persons are very thin without being emaciated. True emaciation means a loss of tissue in every organ excepting the nervous system. In some persons who lose weight there is simply a reduction in the amount of fat: that is not true emaciation. We see an example of this in jaundice, where the loss of weight is due to the destruction of fatty tissue. In fever, the loss of weight is due to a toxic cause. As long as that fever lasts,—and it will last as long as the toxic cause of it exists—we have to deal with a form of malnutrition which it is useless to try to contend with unless we can counteract both the febrile process and its toxic cause.

As the second cause of emaciation I will mention disorders of the alimentary canal. When there is gastric disturbance I have been led to seek first and last for something which the stomach will digest. Unless we can do that, our pepsine, and bismuth, and oxalate of cerium are of no avail. When we come to disorders of the lower alimentary tract, we have to deal with some which are much more apt to produce emaciation than others. I have seen cases of diarrhœa of twenty years' standing, and still the nutrition of the patient was fairly good. On the other hand, we have with dysentery rapid emaciation, because of the fact that the rapid peristalsis interferes with the digestion of the food. Among patients with nervous troubles and the insane, emaciation is not very frequent,

although there are some extreme cases, as in anorexia nervosa, where it is probably due to some disturbance of the digestive tract.

As the third cause of emaciation we have the presence of malignant disease. As the fourth cause I would mention diabetes mellitus, and this, I think, concludes the more common causes. When it accompanies Bright's disease, it is usually due to a complicating gastro-enteritis. In Grave's disease there is striking emaciation, and here I have long contended that there is a toxic cause for it in the intestinal tract. For the emaciation which occurs in chronic rheumatism there is, no doubt, a febrile cause.

DR. L. C. GRAY.—Any man who has been in practice for from ten to twenty years appreciates every day the sad fact that the text-books do not deal with many of the questions which come up in daily life, and for that reason it required a good deal of courage on Dr. Dew's part to read a paper like this, which after all contains so much more that is refreshing and edifying than what we are usually regaled with in the way of gleanings from the journals with a fine tooth comb. In the earlier years of my practice I met with many cases of nervous disease belonging to the sub-acute or chronic type which were exceedingly difficult to treat, and my results were satisfactory neither to myself nor to my patients. After some ten years of varying failures I came across Dr. Weir Mitchell's book on fat and blood making, after reading which I took a trip to Philadelphia, and a brief sojourn there convinced me that Dr. Mitchell was really making the remarkable cures which were being attributed to him. Upon my return home I tried his rest cure, sometimes modified as the case demanded, and in many cases which were hitherto unrelieved the improvement was very rapid.

I cannot agree with the statement made by Dr. Thomson that nervous diseases, with the exception of certain forms of insanity, do not produce emaciation. I have seen marked emaciation in cases of syphilis of the brain; also in myelitis and different forms of paralysis, and I have seen emaciation occur in neurasthenia, although in the latter cases it might have been due to anæmia and gastro-intestinal disturbance.

In the treatment of malnutrition, every possible cause of the condition should be inquired into and corrected, if pos-

sible. There may be anæmia; there may be an insufficient amount of food taken into the body. The nutritious quality of the food, as we understand it, I do not regard as of so much importance. I believe in giving these patients plenty of food, well cooked. Get them in the habit of regarding food as a medicine, and try to impress them with the fact that it is necessary to get a certain amount of food into the stomach each day, no matter how they get it there or whether they have an appetite for it or not.

I think that a very important factor in the production of malnutrition has been thus far overlooked by the previous speakers. Many of these patients, both men and women, get into this condition because they are working beyond their physiological limit. Such patients must be restrained. The amount of work they are doing may have been within their physiological limit at some time in the past, but it is beyond that limit at the present time.

DR. P. A. MORROW.—I have been asked to say something on this subject from the standpoint of the dermatologist. The advocates of the local pathology of skin diseases would perhaps not assign a very prominent place to malnutrition as an etiological factor in their production, but, in common with most dermatologists in this country, I believe that a very intimate relationship exists between disorders of the digestive organs and diseases of the cutaneous system. In fact, there are a large number of skin diseases which are purely symptomatic of impaired nutrition. When we consider the close lines which unite the skin to the general system in its nervous and nutritive relations, it is not surprising that disorders of the general system should be so frequently reflected on the general surface. We find that nutritive disturbances play an important part not only in initiating morbid processes in the skin, but also in perpetuating them. These conditions of impaired nutrition act both as direct and predisposing causes of skin disease. It is very evident that the skin will participate in any impairment of the general nutrition, and by virtue of this impairment its capacity for resistance is diminished. Many causes of skin disease which we recognize as exciting causes would be inoperable were it not for the greater susceptibility due to impaired nutrition. The influence of malnutrition in the production of skin disease is perhaps most conspicuously seen in the extremes

of life. In infancy and in advanced age there is a natural tendency for the skin to become diseased, or undergo degenerative changes, owing to its structure. For example, infantile eczema represents more than 50 per cent. of all cases of skin disease in children that come for treatment. If we look very carefully into the causes in these cases, we shall find that a very large number of them are due to errors of diet. The food is either insufficient in quantity, or improper in quality.

Again, some kinds of skin troubles are peculiar to advanced age. In most of these cases the cutaneous disease is associated with an impairment in the general or local nutrition. As examples of this we have senile pruritus, seborrhœal warts, epithelioma, and certain new growths which tend to undergo degenerative changes.

In many cases of anæmia and chlorosis and in cachectic conditions we not infrequently see various forms of skin disturbance. There are certain diathetic conditions in which the processes of nutrition are interfered with and which are responsible for many skin troubles. The presence of excessive quantities of uric acid and oxalate of lime in the urine is so constantly associated with certain forms of skin disease that we must look upon it in the relation of cause and effect. While such cases can be markedly benefited by local treatment, we must correct the constitutional vice if we wish to effect a radical cure. It is a well recognized fact that scurvy and certain forms of purpura depend primarily on malnutrition, and that the skin lesion is simply a result of the changes in the vascular system. Diabetes may give rise to eczematous or erythematous lesions, and even to more severe lesions, such as gangrene, which are all the result of impaired nutrition.

Many skin diseases cannot be cured until the underlying constitutional condition of impaired nutrition is corrected. We know this is true in certain malignant forms of syphilis. Take for example the form known as the malignant precocious syphilide, which develops almost exclusively in the poorly nourished, or in persons suffering from some cachectic condition. It is impossible to cure such cases by specific treatment as long as the cachectic condition is present. But when the tone of the general nutrition has been improved by good food, tonics, and

other reconstituent remedies the physician may then intervene with specific remedies and with the happiest effects.

DR. R. C. MYLES.—Discussing this subject from a rhino-oto-laryngological standpoint, it would be expedient to consider it, in a causal sense, in two relations,—first, to what extent does malnutrition affect the nose, throat, and ear; second, to what extent do abnormal conditions of the nose, throat, and ear produce or induce malnutrition? The subject is a broad one, and the limited time will allow me to make only a few remarks. In my experience, malnutrition does not affect the nose, throat, and ear nearly as much nor as often as is usually inferred. Excluding the special diathetic diseases, such as tuberculosis, syphilis, gout, etc., malnutrition manifests its injurious effects chiefly upon the nerves and muscles of the larynx, and upon the auditory nerve, especially in regard to tinnitus and passive congestive phenomena. Upon the nose, by reducing its heat producing qualities, by promoting the secretion of a thickened mucus, and at times by producing a peculiar œdema of the dependent portions of the turbinate membranes.

On the other hand, excessive local nutritions, hypertrophic and proliferative, have frequently been the cause of general malnutrition. For instance, in cases of excessive catarrhal secretions malnutrition is induced by great loss of saliva through constant expectoration, or by large quantities of thickened muco-purulent matter entering the stomach and intestines, or by flowing into the bronchi and engendering a chronic bronchitis, which in turn assists in the production of general malnutrition. In my case-book there are many cases of malnutrition where a thorough course of tonics did not produce any beneficial results, but after the nasal stenosis was relieved, adenoids removed, faucial tonsils excised, or granulating conditions of the cavities of the ear or nose corrected, the normal functions of the body were restored, and color, flesh, and strength were gained by the patients.

DR. A. H. GOELET.—I believe it is impossible to follow any hard and fast rules in the correction of the condition known as malnutrition. Each case must be treated on its own merits. The clue to the treatment necessary must be obtained by an examination of the blood and the bodily

excretions. One agent of inestimable value in the correction of this condition has not been mentioned by the speakers who have preceded me. I refer to electricity. This is a remedy absolutely without comparison in this condition. In many cases where I have employed it for the correction of some local condition of the female pelvic organs, there has been an improvement in the general health entirely out of proportion to what could be expected from the improvement of the local condition. The faradic, alternating, and statical currents are particularly useful. In many instances it is impossible to get any good effect either from our remedies or diet unless we first stimulate the secretions of the body to a normal activity. Electricity will do this unquestionably. It stimulates both the secretions and excretions in a manner which cannot be accomplished by any other means.

DR. M. L. RHEIN.—I wish to present another side of this question, which has not been touched upon to-night. To the stomatologist the subject of malnutrition possesses as much interest as to the rhinologist or dermatologist. If malnutrition plays such havoc with the skin, it is bound to do even more serious injury to the mucous membrane of the mouth. It is a well known fact among us that more teeth are lost through the effects of malnutrition than through decay or any other form of disease the teeth are subject to. This is due to the fact that it produces such a low vitality in the structure surrounding the sockets that sooner or later it breaks down and pus pockets form from which the pus continually dribbles at the neck of the tooth.

This induces a constant low form of septicæmia, and added to the local irritation of the alimentary tract there is present a powerful resistance to all general remedial agents. This local pathological manifestation around the roots of the teeth is termed pyorrhœa alveolaris, and the local treatment of this disease is a natural necessity. In curing this condition the stomatologist should also not lose sight of the importance of correcting any defect in the ability to properly perform the function of mastication.

In closing I desire particularly to call your attention to the important place the condition of the mouth holds in early diagnosis of functional diseases where malnutrition plays an important part. The lowered vitality, anæmic appearance of gums, together with the acid condition of

the follicular secretions, even if pus has not already exuded from the margin of the gum, will be apparent before other marked symptoms have been noticed. It has frequently fallen to my lot after an examination of the gums to be the first person to inform them of an incipient attack of Bright's, diabetes, or other functional disease. Can we underrate the value of an early diagnosis in such cases?

DR. DEW (closing the discussion).—I have nothing to add to my paper, excepting to say that I am particularly pleased with the many interesting suggestions that have been offered, and I think we will all profit by them. In what I have said in my paper I do not mean to be absolute, but simply to suggest.

Stated Meeting, March 21st, 1895.

DISEASES OF THE EYE DEPENDENT UPON GRIP.

By THOMAS R. POOLEY, M. D.

If it be true that "uneasy lies the head that wears a crown," it is certainly more true that uneasy lies the head that has the grip. *La grippe*, or as we shall designate it in the vernacular, "grip;" for, as the disease itself is vernacular, there seems to be no good reason for adhering to the French when our own language is just as forcible, and means the same thing.

Grip may be briefly defined as a specific, epidemic, self-limited febrile disease, probably dependent upon a bacillus or microbe, as indeed are all other diseases. In fact, we may divide all acute, epidemic, and contagious diseases into those in which the bacillus responsible for their existence has been found and those in which it has not yet been found. Certain bacteriologists claim to have found a microbe constant to all the secretions of grip, but have not been able to reproduce it. Periodical outbreaks of grip may be expected whenever favorable climatic conditions afford an opportunity for the development of the bacillus by which it was caused. We all remember the epidemic which visited New York with such virulence from 1889 to 1891. Early in January, and up to the last month of the present year, the disease was very prevalent in New York, but from January 25 to February 10, it rapidly decreased; and although it still exists, it is by no means as prevalent, and the attacks are not so severe. In London and Berlin, however, the disease is widespread and the mortality is

very large. In Berlin recently, the total number of cases was estimated at from thirty to forty thousand, and in both London and Berlin, it embraced in its list a number of distinguished personages.

During the prevalence of the grip in New York in 1891, a grip symposium was held in the New York County Medical Society, in which short papers were read, and a general discussion ensued. The President, Dr. Fox, asked the writer to take the subject of "The Relation of the Grip to Eye Diseases," and treat of it in a discussion which should last only five minutes. It was found that the space of time allowed would not even permit the enumeration of the diseases which, by a reference to the literature of the subject, had been attributed to the grip. The literature, too, of that epidemic was very voluminous, but a recent search through both the general and special journals shows a great falling off in the number of papers on this subject. As naturally as the hand divides into fingers, or the tree into branches, seems the disposition to account for the occurrence of all diseases—not only of the eye, but other organs as well—by the prevalence of any epidemic. Although it may be admitted that "grip is very versatile in its handling of the human body, and hardly any part of it has been overlooked in making his rounds," it must also be admitted that the evidence of some of the diseases attributed thereto, will be found upon examination, very meagre, and to be more a fashion or a fad than a fact.

In beginning the enumeration of the various affections of the eye, attention may be called first of all to the absurd statement made by some pathologists, that the grip poison first finds its lodgment in the eye; and if the conjunctiva is in good condition, the chances of an attack are slight. From such an absurd starting point, then, no wonder need be expressed that nearly all forms of inflammatory diseases of the eye, as well as those of the nervous tissues thereof,

have been attributed to the grip. It is too well known to need comment or further proof, that very many affections of the eye depend upon other constitutional diseases, and why not then the grip as well? But, in the various publications of the long list of eye diseases caused by grip, many are totally unsustained by proofs. In this paper, therefore, we shall briefly enumerate the affections of the eye which have been declared to be caused by grip, and seek to show what reason can be adduced for such an assumption, without a tiresome detail of cases, which will, however, be needed to sustain or disprove the cause.

The following diseases of the eye have been described by various authors as due to the grip: Badal[1] mentions catarrhal and follicular conjunctivitis, blepharitis ciliaris, eczema, hordeolum and inflammations of the sac; Copez, of Brussels, croup and diphtheria; Galezowsky,[2] episcleritis; Badal, eruptive and suppurative keratitis, iritis, and irido-choroiditis; Guttmann,[3] keratitis dentritica; Hosch,[4] purulent irido-choroiditis; Laquer,[5] embolic irido-choroiditis and embolism of the central artery of the retina; Hosch, suppurative hyalitis: Adler[6] and Eversbusch,[7] acute glaucoma; Fuchs,[8] Bergmeister,[9] and Schapringer,[10] suppurative and non-suppurative tenonitis; Valude, phlegmon of the orbit.

The list is taken from that collected by Dr. John E. Weeks, and embodied in a paper on "The Grip as a Cause of Retro-Bulbar Neuritis, and Other Ocular Nerve Lesion," read before the Section on Ophthalmology and

[1] Archiv d. Opthal, 1890, p. 146.
[2] Reported by Gazis, Rev. d'Opht., 1890, p. 586.
[3] Berlin Klin. Woch., 1890, p. 111.
[4] Correspondenzbl. f. Schweiz. Aerzte, 1890, p. 163.
[5] Klin. Monatsbl. f. Augenheilkunde, 1890, p. 195.
[6] Wien. Med. Woch., 1890, p. 145.
[7] Münchener Med. Woch., 1890, Nos. 6 and 7.
[8] Wien. Klin. Woch., 1890.
[9] Wien Klin. Woch. 1890, p. 11.
[10] Medical Record, 1890, p. 679.

Otology, of the New York Academy of Medicine, May 18, 1891. This list might be enlarged, if it could subserve any useful purpose by doing so, but will suffice to show what a number of different forms of eye disease have been described by these authors as caused by grip. This list, as it will be observed, deals only with the inflammatory diseases affecting the various tissues of the eye. It is indeed difficult to imagine the existence of many of these affections during an attack of the grip, except as an accidental occurrence. Among these we may class blepharitis ciliaris, eczema, croup and diphtheria of the conjunctiva, and acute glaucoma. In regard to the latter, however, the writer may merely mention that his colleagues, Drs. Born and Webster, have both recently reported to him orally the occurrence of an attack of acute glaucoma during one of grip; but neither thought the occurrence of glaucoma at this time was anything more than a coincidence. It is well known that any condition of the system that produces debility and depression, favors the outbreak of glaucoma; even grief and sorrow, and the consequent depression, often bring about such a result in eyes predisposed to this disease. The extensive and severe inflammation produced in the upper air passages, and the sinuses which are in more or less direct communication with the eye, such as the ethmoidal and frontal, make it highly probable that not only are simple and purulent forms of disease of the conjunctiva produced by grip, as the frequency with which these forms of conjunctivitis accompanying the disease demonstrate, but the severity and location of the severe frontal headaches also point to the probable implication of the accessory cavities. In one case, which occurred to a distinguished surgeon of this city, the frontal sinuses were involved to such a degree as to cause an empyema, which had to be surgically treated. That the forms of the disease above enumerated are not directly caused by the grip, but are present in most of the

cases, there seems to the writer to be little doubt, and it is a question worthy of considerable thought, whether the implication of the accessory sinuses may not even be responsible for the extension of the inflammatory process from thence to the orbit, thus giving rise to the extension of the disease to the tissues, and to the intraorbital part of the nerve. The implication of the latter, as we shall see, has been explained in quite another way. It seems, too, to the writer not illogical to consider an attack of grip, in some degree, at least, responsible for the occurrence of an attack of acute dacryo-cystitis, or phlegmon of the sac, especially in patients subject thereto by stenosis of the duct, or who have had similar attacks. Severe swelling and inflammation of the mucous membrane of the nose and throat induced by disease are mainly responsible therefor. Such a case the writer now has under care, occurring in a severe attack of grip in a patient who has had such attacks before; but in this, as in most such cases, there existed beforehand either closure or stricture of the nasal duct.

In order to add as far as possible to the practical interest of this paper, it has been the object of the writer to acquire, as far as he could, confirmatory evidence from the experience of others as well as from his own clinical observations, bearing on the occurrence of eye diseases attributed to the grip. These cases have not been hitherto published, but have been kindly sent to him by his colleagues; and it would seem that this line of investigation would be more interesting than a further reference to literature.

In regard to corneal diseases being due to this affection, it should be said that we have analogous diseases which have already been put down as giving rise to them; and it is well known that any severe disease which produces a lowering of the nutrition and vitality may be the cause of the various forms of inflammation and ulcer of this poorly nourished structure. It must not be lost sight of that the

well-known effects of this disease on the nervous system may act in producing the eruptive form of keratitis. Dr. Webster has kindly sent to the writer a case of corneal disease, said to be due to grip, and in the letter accompanying the case, he says he knows it to date from the grip. The following are his notes:

March 8. Patient, male, thirty-five years of age. Three weeks ago had a severe attack of grip, lasting two weeks, with all the usual symptoms, frontal headache and neuralgia being the pronounced symptoms. Among the earlier symptoms were pain over the eye, especially severe over the left orbit. This eye was red, painful, and intolerant of light. The physician in attendance saw a film over the cornea. There was still some circum-corneal injection, photophobia, lachrymation, and pain, although only a fading macula of the cornea on the inner side of the pupil marked the site of the former corneal disease. There was no evidence of any herpes of the brow. A few days later, March 11, a case was observed in my own clinic, in a girl of eleven, who had grip about a week before. She was sick in bed for five days. On the first day of getting up, the left eye was red and painful, with marked circum-corneal injection, but no pain, and only slight photophobia. There was well-marked superficial punctate keratitis, consisting in the presence of a number of gray spots over the cornea—in short, such a form as often affects young persons, has been recognized as occurring in conjunction with catarrh of the air passage and therefore we may assume in the same way, related to grip, in which the catarrh of these passages is so universally present. Any disease which has hitherto been held as liable to happen in such conditions, may be assumed to be due to the grip. In all of the cases collated by Weeks, referring to corneal disease, the forms of the disease were various, and there would seem to be some reason for placing them in causal relation to the grip.

In considering whether any of the suppurative processes mentioned in the list above quoted can be considered as due to the grip, we must admit that if the affections of the conjunctiva are due to the pathogenic agency which gives rise to it, other forms of inflammation are likely to be due to the same cause, and, as has been shown by Fuchs, the micro-organism described by Klebs,[1] Weichselbaum,[2] Babies, and others, was present in at least one of the cases of tenonitis. But all this requires further proof. The cases of tenonitis referred to by Weeks are the only instances referred to the grip, so far as the writer knows, and need further confirmation. In this list of rare occurrences, too, may be placed phlegmon of the orbit. That inflammation of the uveal tract may occur in many diseases, we have evidence. Suppurative choroiditis, as is well known, not infrequently complicates severe forms of pyæmia and puerperal fever. A suppurative choroiditis, which is likewise of metastatic origin, occurs in rare instances in typhoid fever, cerebro-spinal meningitis, scarlet fever, measles, and other diseases, and in which category Fuchs includes influenza, of which grip is only a severe form. It may be in this way, or by a transfer of the inflammation from behind forward from phlegmon in the orbit, and in thrombosis of the orbital veins, that we have the uveal tract involved in grip. Since in metastatic affections of the eye, the infecting plug may occur, not only in the blood vessels of the choroid, but also in those of the retina, it is best to comprise, according to Fuchs, this class of cases under the name of "metastatic ophthalmia."

Besides this, the cases which have been already referred to in the literature of the subject, Dr. Joseph A. Andrews has kindly allowed the writer to make use of the following cases, five in number, all occurring in children, and believed to have been related to an attack of the grip.

[1] Deutsch. Med. Woch., 1890, p. 140.
[2] Wien. Klin. Woch., 1890, p. 104.

The ages were between seven and nine years. One of the children he saw only once; another several times; and one he still has under observation. In the case now under observation, the family doctor had called the disease with which the child was ill, the grip. It was not, however, a severe attack. When the child was getting better of the grip the eye trouble was noticed. The eye was red and painful—no headache. Seen a month after, the eye was affected, the pupil was contracted, with posterior synechia and circum-corneal injection. Eyeballs sensitive to pressure; also some objective pain; eyeballs soft. When pupils were dilated with atropine, a white, shiny body was seen behind the lens, which had in places a slightly pinkish hue, but no vessels could be made out. It looked like what has been called pseudo-glioma. The child was under observation a month. Only one eye affected; the other normal. There was no other cause for the eye trouble except the grip. Careful inquiry was made on this point. In one case there had evidently been non-suppurative pan-ophthalmitis. The eye trouble had developed during the early stages of convalescence. In the third case, there was irido-cyclitis; the sight was entirely gone. Only one eye was affected in all of the cases. The fourth case was like the first of which I have written. The fifth case was only seen once. The eye was soft, and undergoing phthisis. In this, as in all the other cases, the eye trouble was first noticed during convalescence, and there did not appear to have been symptoms of head troubles during, or subsequent to, the attack of grip. All the children had coughs.

Andrews comments upon this most interesting series of cases as follows: "Of course, I tried to exclude trauma and other causes than grip, nevertheless other causes may have operated to produce the eye trouble. I do not see why we should hesitate to hold the grip responsible for the complications of which the children I have referred to fur-

nished illustrations. The same conditions have been recognized very long ago as occurring in measles, typhoid fever, etc."

It is to be hoped that Dr. Andrews will publish these cases in detail. Unless they had been observed by such a painstaking and accurate observer, one might feel inclined to question the accuracy of the observations as to their etiology; but there can be no doubt but that all possible precautions were taken to avoid any error, or overlook any other cause. The fact, however, of so many cases coming under the observation of the same observer in so short a space of time would suggest the possibility of their being due to cerebro-spinal meningitis. The writer has never seen an example of inflammation of the uveal tract which could be possibly attributed to the grip as a cause, but he does not see why the complication of embolic or metastatic inflammation of this tract may not just as well be due to grip as to any of the other acute infectious diseases of which we have numerous authentic examples, and which are now considered as orthodox.[1]

We now approach the consideration of the subject of great interest and importance, and upon which the data that have been collected seem to be more uniform and reliable than in those we have already gone over—the manifestations of grip on the nervous system, as they affect the eye, and to which we shall now invite your attention. That grip has a great affinity for the nervous system is shown by its effects on the sensory nerves, producing defi-

[1] Since reading this paper a letter from Dr. Alt has been received, in which he says he published a short article on "Eye Affections after the Grip," in the February number of the American Journal of Ophthalmology for 1890. In a discussion two years ago on the same subject in the St. Louis Medical Society, he reported that in the winter of 1892, he saw seven cases like those of Andrews. In three, both eyes were destroyed; in the others, one eye only. They all happened in children, and they resembled absolutely a metastatic (septic) choroiditis. He adds: "I think that the case published in 1893 or 1894, by Dr. S. Pollak, in the American Journal of Ophthalmology, with microscopical examination by myself, was of the same nature."

nite pains experienced in various parts of the body, such as severe pain in the limbs and back, and severe frontal and temporal headaches. The latter pains have not been inaptly described by a humorous journalist as resembling those caused by a "misfit skull." There are also the effects on the nerves of special sense resulting in the disturbances of taste, smell, hearing, and vision; the effects on the motor nerves, giving rise to the paralyses which have been observed; and the effects on the vaso-motor nerves, producing the sensation of chilliness, and later, congestion of the lungs, mucous membranes, and other tissues, first by contraction of the lumen of the blood vessels, and later, by vaso-motor paresis.

Although the effect upon the brain and central nervous system has been sufficient to cause temporary insanity, and even death, the manifestations which have been noticed in the nervous system are usually in the form of peripheral neuritis. Among the conditions which have been observed, referable to this cause, are to be included, peri-orbital and orbital neuralgia, as well as headache of a more general form; paralysis of accommodation, and of the extrinsic muscles of the eye; one-sided ophthalmoplégia, externa and interna; paralysis of the cervical sympathetic of one side, with prominence of the globes; paralysis of the facial nerve; retro-bulbar neuritis and papillitis. Of the almost constant symptoms, orbital neuralgia and migraine, we are all aware, and need not therefore dwell upon them. Whether, however, this is not in part due to the congestion of the accessory sinuses, the frontal and the ethmoid, as well as the affections of the peripheral nerves, is deserving of more attention. Anæsthesia of the cornea has been noticed by Galezowski; Gazis and Bergmeister report the conditions in one eye in some and in both eyes in other cases, and there was complete return of sensibility in a few days. Paralysis of accommodation has been quite frequently observed.

In 1890, in discussing a paper by Dr. Louise Fiske Bryson, read before the Section on the Theory and Practice of Medicine of the New York Academy of Medicine, January 21, 1890, the writer said there had also been noticed paresis of the accommodation, and that such was a noticeable feature in his own case after a severe attack of the grip. Dr. Weeks, in his paper, quotes two cases which are quite typical, but inasmuch as we have two equally conclusive observations coming under our own notice, we need merely refer to these. One is from the clinic of Dr. Webster, kindly reported to the writer by his assistant, Dr. Blodgett, and here made use of by his permission:

A male, aged thirty-five, grip two years ago, and again three weeks ago. For the last three days complains of blur over everything. Wide pupils. R. V.$=^{20}/_{xx}$; L. V.$=^{20}/_{\infty}$; with + 1 D., both $^{20}/_{xx}$. Reads Jaeger No. 1 with + 4 D., at 14 inches. Diagnosis: Paralysis of accommodation from grip.

The other case, one of monolateral paralysis of accommodation, came under the writer's care while he was preparing this paper, March 11, at the New Amsterdam Eye and Ear Hospital:

A male, aged thirty-eight; had grip three weeks ago. R. V.$=^{20}/_{xl}$ with + 1.25 D. Reads No. 13 at 19 inches. With + 4 D., reads No. 1 at 8 inches. L. V.$=^{20}/_{xxx}$, with + .075 D. $=^{20}/_{xx}$; reads No. 1 at 8½ inches without a glass. Diagnosis: Paralysis of accommodation of right eye from grip.

Dr. Andrews has also seen bilateral paresis of accommodation following grip as after diphtheria. In the latter class of cases, the patient has always recovered the use of accommodation. Such observations as those already published, and the facts here adduced, seem to show conclusively the occurrence of paresis and paralysis of accommodation after grip, as also in other acute and chronic diseases, such as diphtheria, typhoid fever, syphilis, and the

like. It will also be necessary to carefully exclude the other well known causes, especially before confirming the diagnosis of grip. The fact of this disease affecting the accommodation may also be looked upon as confirmatory of its affecting other nerves, as is shown in the involvement of other branches of the third nerve.

Paresis of the Extrinsic Muscles.—Paresis of the nerves which innervate the muscles of the eye have been noticed by various authors as among the complications of grip. In a majority of all these cases, there is a sensation of pain in the movements of the eye, and deep seated in the orbit. To these I may add, as in my own experience, a very severe and decided asthenopia is observed on any attempt to use the eyes. That such subjective symptoms as these usually eventuate in a decided paresis, or even paralysis of the muscle of accommodation, as well as of the ocular muscles, is not to be wondered at, as the following facts attest.

Paralysis of the extrinsic muscles has been observed by Badel, who reports two cases as follows:

CASE I.—Male, aged thirty-three; no rheumatism or syphilis. Severe attack of grip with cephalalgia, lasting three or four days. During convalescence patient began to see double. Examination showed paresis of the muscles supplied by the third nerve on the right side, amounting to complete paralysis of the superior rectus. No mydriasis or loss of accommodation. Recovery took place slowly.

CASE II.—Male, aged fifty-seven; no rheumatism or syphilis. During recovery from an attack of influenza, which lasted six days, and was accompanied by severe headache, patient saw double when looking at a distance. Examination showed paralysis of the right externus. At the end of five days, the patient only saw double by putting a colored glass before one eye. Recovery was rapid and complete.

As a point of differential diagnosis between paresis of the ocular muscles dependent upon grip and those due to syphilis, the rapid recovery in the former is worthy of note.

Dr. H. S. Oppenheimer found paresis of the inferior rectus in one case in which there was also retro-bulbar neuritis, and which I shall refer to under this head. Uhtoff, from an extract by G. Linn (in *La Semaine Medicale*, 1890, p. 30), mentions a case of paralysis of the accommodation complicated with progressive ophthalmoplegia externa, and with symptoms of bulbar paralysis. Grieff reports a case of paralysis of the left sympathetic, enlargement of the left thyroid gland, prominent globes, heart action rapid and irregular in which ptosis of the right upper lid developed.[1]

In closing this category of nerve lesions observed as a consequence of the grip, we shall have to include affections of the optic nerve. Neuritis, I believe, was first noticed in this city by Weeks during the epidemic of this disease in 1890, and described by him in a paper read before the Section on Ophthalmology of this Academy. In the discussion which followed the reading of this paper, the present writer remembers to have expressed his doubts as to the case reported being due to the grip; but in view of the many similar observations since made, he is inclined to think that he may have been mistaken in this opinion, for so many other observers have sustained the view that grip may give rise to both retro-bulbar neuritis and papillitis, and he has also observed one case, yet to be referred to, which could not be attributed to any other cause. Weeks found that the retro-bulbar form of neuritis has been most frequently observed, but that papillitis had been noticed in five cases, of which he gives a brief *résumé*—two by Gazis and three by Guttmann and Vignes.[2] These cases are briefly reported by Weeks. The cases of neuri-

[1] A similar case has been reported to the writer by Dr. W. E. Chase of New York, occurring after grip two years ago. Soon after, the patient, female, aged 28, noticed prominence of eyes, which increased for nearly a year, since which time there has been well-marked exophthalmos of both eyes. She is in good health. There is no cardiac trouble and no thyroid enlargement.

[2] Rev. d' Opht., 1890, p. 402.

tis were characterized by hyperæmia, œdema, and exudation in the vicinity of the disc, and all other causes than grip were carefully excluded. The essential features are referable, therefore, to inflammation of the nerve—neuritis.

Dr. Oppenheimer has furnished the writer with brief notes of two cases of neuritis, and one of retro-bulbar neuritis. The former are as follows:

Male, aged thirty-five, just recovered from grip with severe cough; has never been ill otherwise. Great ecchymosis of lids and conjunctiva, both ocular and palpebral, which came on over night. Positions of the eye normal, and pupils normal. Left eye, disc hazy, veins greatly enlarged. L. V.=$^{20}/_{LXX}$; R. V.=$^{20}/_{XX}$. Diagnosis: Neuritis of left, probably due to bleeding in the sheath of the nerve. No color scotoma.

H. D., male, aged twenty-seven; has pain in the muscles of the chest and back from grip. Yesterday he noticed the right eye growing dim. In and around the optic disk of this eye are splashes of hæmorrhage with exudation at the edge of the nerve. Is pronounced by an expert perfectly healthy with perhaps slight anæmia. No venereal disease. No other sickness since childhood. Vision=$^{16}/_{CC}$. Does not drink; other eye normal; no color scotoma.

To these cases of papillitis the writer can add one more which has no other ascribable cause than the grip. It occurred in a female, twenty-one years of age, seen in the New Amsterdam Eye and Ear Hospital, January 10, 1895. About three weeks before, she had a severe cold, which confined her to the bed for three days, with severe pains over the body, and with a cough. She still has the cough and headache. Three days ago, noticed a blur before the right eye. V.=$^{20}/_{LX}$. Ophthalmoscope shows hyperæmia and exudation at the margin of the disc; no color scotoma; other eye normal. Field of vision not taken.

In the other cases reported, no papillitis was found,

although it may have existed in the earlier stages of the disease. D'Eperon reports six cases; Bergmeister two; Hansen and Weeks one. In this series of cases, papillitis of an active kind was wanting, and the ophthalmoscope showed either ischæmia of the retinal vessels, or a pallor of the part or whole of the disc, indicative of atrophy, and the inflammatory condition supposed to be responsible was assumed to be behind the intra-ocular end of the nerve—retro-bulbar. They were, moreover, characterized, besides loss of vision, by restriction of the visual field, and form and color scotoma. Dr. Andrews says he has never seen papillitis, but he has seen retro-bulbar neuritis dependent on the grip.

The only case of the series which I shall report is the one the notes of which Oppenheimer has been kind enough to send me:

Male, aged thirty. Felt everything moving before his eyes one afternoon while at work. The next morning he was blind in his right eye. The right eye was prominent and painful when pushed back in the orbit; the pupil was dilated *ad maximum;* the media clear; nerve and retina between the macula and disc, hazy. Vision was $^{8}/_{cc}$; no color scotoma. Eye painful on looking upwards, and double vision, due to paresis of the inferior rectus. Diagnosis: Retro-bulbar neuritis. Has just recovered from the grip. No other complaint except great nervousness and a rapid pulse. Denies syphilis, and has no trace of it.

It will be noticed that in nearly all, if not all, of these cases the eye affection develops in the later part of grip, during convalescence, and as in affections of other kinds in which the eye disease develops, is rather to be considered a sequela than a symptom of the disease itself. It would seem from the published account of the cases that caution was taken to exclude tabes, syphilis, and other diseases, as well as the abuse of alcohol and tobacco. Weeks thinks that in those cases which he reports, all

other factors may be excluded by (1) the large proportion of females affected; (2) the character of the scotoma observed; (3) the sudden loss of vision and the uniform history of its occurrence in grip; (4) the absence of improvement in vision under conditions which uniformly produce such improvement in amblyopia from the abuse of alcohol and tobacco. Still, we must all have in mind how difficult it is to exclude this latter factor.

As a result of the foregoing, it would seem to be warrantable to summarize the following conclusions:

1. That the eye complications following grip are comparatively rare—a fact which becomes apparent when we remember the immense number of cases of grip which have occurred during the past five years, and the exceedingly small number of cases of eye trouble reported.

2. Many of the cases reported as due to the grip are fanciful, and need more substantial proof.

3. Grip may affect the eye by inflammatory process, or by invasion of the accessory sinuses.

4. It may affect the nervous tissues.

5. The inflammatory affections involve especially the conjunctiva, the uveal tract, tissues of the orbit, and perhaps the fibrous capsule of Tenon.

6. In some of these cases the extension is by continuity, and in others by metastatic or embolic processes.

7. The nervous apparatus of the eye is especially liable to become involved by paresis of accommodation or of the extrinsic muscles of the cervical sympathetic, by papillitis and retro-bulbar neuritis, and also anæsthesia of the cornea.

8. Before assigning the grip as a cause of any of the eye complications enumerated, careful and thorough scrutiny is necessary to exclude other causes, especially in affections of the nervous tissue—syphilis, tobacco, alcohol, etc.

THE AURAL COMPLICATIONS OF INFLUENZA.

BY EDWARD B. DENCH, M. D.

In considering this subject we have first to remember that epidemic influenza belongs to the class of acute infectious diseases. The constitutional poison effects certain general changes common to all acute infectious diseases, and therefore the influence of this malady upon any particular organ of the body resembles that of any other acute systemic infection. According as the toxic principles expend themselves either upon the upper air passages or upon the nerve centers, the aural complications usually manifest themselves as affections of the conducting mechanism or of the perceptive tract. It is still a mooted question whether the effect produced upon this organ of special sense is due to the action of the specific germ of the disease, or whether the development of certain toxines in the blood renders the tissues more vulnerable to the action of certain micro-organisms found in similar local conditions, not depending upon influenza. When the nervous system is principally involved the action upon the organ of hearing may be indirect; some disturbance of the vaso-motor center so affecting the circulation in either the external or middle ear, or in the labyrinth, as to impair or pervert audition without causing an inflammation.

For convenience of classification we may consider the effects of this disease upon the external auditory meatus, upon the middle ear and the Eustachian tube, and upon the labyrinth and trunk of the auditory nerve. Why in certain cases aural complications play so prominent a part, while in others the ear remains unaffected, undoubtedly depends upon some pre-existing local condition. The same

cause is often operative in determining the particular part of the auditory apparatus affected.

It is probable that an external otitis either of the circumscribed or diffuse variety directly complicating epidemic influenza, is rather rare. Such complications can usually be explained upon the theory that the intense congestion of the vessels of the head which the disease causes, renders the tissues of the canal especially susceptible to infection.

In one instance which came under my own observation, there was an extensive growth of aspergillus in either external auditory meatus, following a severe attack of influenza. No doubt the spores had been present in the canal for an indefinite period, but the intense local congestion which the constitutional disease caused constituted a condition that accelerated their growth sufficiently to produce severe symptoms.

The development of vesicles upon the walls of the meatus and upon the surface of the drum membrane has also been observed in cases of this constitutional malady and may, I think, be considered as trophoneurotic in origin and of the same nature as herpes. These vesicles are sometimes filled with clear serum, while in other instances they are filled with blood.

When we come to consider the middle ear and the Eustachian tube the importance of a previous pathological condition of the upper air-passages must be borne in mind. Adenoid growths in the nasopharynx, nasal obstruction, enlarged faucial tonsils, etc., are all operative in causing involvement of the middle ear. The complications vary in severity from a simple closure of the Eustachian tube on the one hand, to a severe suppurative otitis media with mastoid involvement upon the other. Why, in one instance, the patient should escape with a simple tubal congestion, and in another case should suffer from acute suppuration, is still a matter of conjecture. The most

plausible explanation is that the intensity of the local complication depends upon the degree of systemic infection. It is also probable that some pre-existing local condition may have rendered the parts particularly susceptible to infection. It is doubtful whether the disturbance is ever caused by the local action of the specific microbe of influenza. Thus in a simple tubal catarrh we have a relaxation of the vessels of the Eustachian tube and an intense congestion and œdema of its lining membrane completely occluding the canal. The effect upon the middle ear depends entirely upon the reduction of atmospheric pressure in the tympanum. This constitutes the mild type of middle-ear complication in influenza. Carried one step further, the vessels within the middle ear dilate and there is transudation of serum into the tympanic cavity. This fluid forms an exceedingly good culture medium for the development of bacteria which may find their way into the cavity through the tube. In the more virulent cases the connective tissue structures lying in the upper part of the tympanic cavity become susceptible to the action of pyogenic bacteria, and then we have a severe inflammation of this connective tissue, or an acute purulent otitis. The middle ear then will be the site of a simple transudation, a mild catarrhal inflammation, or a purulent inflammation according as the power of resistance of the tissues is diminished by the action of the poison circulating in the blood.

The aural complications observed during the various epidemics seem rather to bear out this view. In the first epidemic in this country, the aural complications were far more grave than in the epidemics of the last two years. The involvement of the mastoid process was exceedingly common and usually so rapid in progress as to necessitate operative interference. During the past two years simple tubal congestion or a congestion of the tube and middle ear with an effusion of serum into the tympanic cavity,

has been more common than an acute purulent inflammation.

The symptoms to which these various conditions give rise are so well known as to scarcely demand remark here. It is well to remember, however, certain appearances which enable us to decide between an acute purulent process and simple catarrhal inflammation or tubal and tubo-tympanic congestion. An acute purulent inflammation within the tympanum must of necessity involve the connective tissue structures lying within the tympanic vault, hence otoscopic examination reveals in the early stages an intense congestion of that portion of the membrane lying above the short process of the malleus, while the remainder of the membrane may be perfectly normal in color. This may be the only deviation from the normal standard and yet the patient may complain of intense pain in the ear. Viewed a few hours later we find the upper portion of the drum membrane bulging into the canal, frequently to such an extent as to hide the short process, while the superior wall of the meatus seems to be continuous with this red, bulging portion. The tumefaction may be so great as to partially hide the part of the membrane below the processus brevis, and the inflammation may advance so rapidly as to cause but little deviation from the normal standard of color in the inferior portion of the visible drum membrane. An inflammation here is exceedingly prone to be followed by mastoid inflammation unless dealt with promptly. It is always wise to incise this tumefied tissue immediately, the knife being inserted close behind the short process of the malleus, carried backward to the periphery of the drum membrane, and then outward upon the canal wall. The object of this incision is to deplete the tissues of the tympanic vault and thus to abort the inflammation. If instituted early this plan seldom fails to prevent extension to the mastoid process. Even after disintegration has taken place within the tympanum, and

pus is evacuated upon incision, the free use of the knife as described not infrequently prevents extension to the mastoid process.

The picture presented by a simple tubal catarrh, tubotympanic congestion, or an acute catarrhal inflammation is very different. In tubal catarrh the changes seen upon speculum examination depend simply upon the rarefaction of the air within the tympanum. The drum membrane is drawn inward, the short process of the malleus is prominent, the long process fore-shortened, and the transverse diameter of the fundus of the canal is apparently increased, owing to the displacement inward of the posterior segment of the membrane. There is no congestion of the membrane and no increased vascularity of the canal, the tissues above the short process of the malleus appearing perfectly normal.

Where both the tube and middle ear are congested we have, in addition to a displacement of the drum membrane inward, a turgescence of this structure itself limited to its periphery and to the manubrial plexus. If there has been a transudation of serum into the cavity we may recognize either the fluid line if the membrane is sufficiently thin, or air bubbles may be seen in the fluid appearing as bright points which change their position upon forcing air into the middle ear. A sign of practical importance is that the region of increased vascularity is confined to the region of the larger blood vessels,—in other words, it is limited to the periphery of the membrane and to the manubrial plexus. There is no injection of the membrane itself, but simply a dilatation of the normal venous channels.

In an acute catarrhal inflammation, the lower portion of the tympanic cavity alone is involved; the drum membrane is uniformly reddened, and presents an appearance very different from that seen when the process is of an acute purulent character. In this last condition we have

already stated, that the parts first affected were situated above the short process of the malleus. In an acute catarrhal inflammation the entire tympanic membrane, particularly the membrana vibrans or that part lying below the short process of the malleus, at first appears pinkish, and then of a deeper red color. The membrane bulges into the canal as the fluid accumulates behind it, and this fluid is sero-mucous in character, and not purulent. It is seldom that the increased vascularity reaches such a stage as to completely obscure the short process of the malleus, and often the entire outline of the manubrium can be made out.

Here also an early and free incision is demanded to prevent a subsequent extension to the cellular tissue in the tympanic vault. When the effusion is not excessive it may be absorbed, the process being aided by inflation. If there is a large quantity of fluid it must be evacuated by a free incision. Paracentesis or simple puncture of the membrane is not sufficient to evacuate the fluid in these cases. The incision should lie in the posterior segment, should be free, and should follow the periphery of the membrane, extending from the lower and posterior portion of the membrane upward to the posterior fold. This not only evacuates the inflammatory products, but also thoroughly depletes the tissues and favors a rapid return to the normal condition. It is well in making this incision to carry the knife inward until the internal tympanic wall is encountered, and to divide the mucous membrane covering this, as well as the drum membrane, in order to more completely empty the engorged vessels.

Where we have to deal with tubal obstruction, or with a tubo-tympanic congestion, catheterization is usually sufficient to relieve the condition; if, however, there has been an extensive effusion it may be necessary to evacuate this by incision as in the cases of acute catarrhal inflammation.

To recapitulate, the gravity of the middle-ear complication

depends upon the portion of the tympanic cavity involved, and this in turn is determined by the appearance of the parts upon otoscopic examination. Those cases in which the tympanic vault is affected are of a grave nature, rapid in progress, and frequently extend to the mastoid; they are characterized by redness and bulging of the membrana tympani above the short process of the malleus, the superior wall of the canal appearing to sink downward so as to partially hide the drum membrane, and thus reduce the vertical diameter of the fundus of the canal.

Those cases involving simply the Eustachian tube, or the tube and lower portion of the middle-ear cavity, are of a less serious nature and are not as a rule followed by mastoid inflammation. Here the speculum shows no diminution in the vertical diameter of the drum membrane and that portion of the membrane above the short process of the malleus is not involved, the lower portion of the tympanic cavity being alone affected.

So far we have considered those cases seen in the early stages and before perforation of the drum membrane has taken place. When the drum membrane has ruptured spontaneously, the perforation is usually small and must be enlarged in order to effect free drainage. An investigation of the discharge from the middle ear in these cases has shown the presence of the ordinary bacteria of suppuration, and according to Scheibe[1] a bacillus of a particular form which is found only in cases following influenza, and which seems to be identical with the influenza microbe observed by Pfeiffer, Kitasato, and Canon. As the clinical history of these cases differs only in degree from that of an acute otitis media arising from other causes, it seems more probable that this particular bacillus is not the cause of the inflammation, but that by its action the tissues are rendered more susceptible to the ordinary bacilli of suppuration.

[1] München. med. Wochenschrift, 1892, No. 14.

The treatment of the discharge from the ear which appears either after spontaneous rupture, or after incision, is a matter of importance, to prevent either mastoid inflammation, or the infection of the external auditory meatus. The parts must be kept thoroughly cleansed, and this can best be done by frequent irrigation. Boiled water, or a mild antiseptic solution, is to be employed, the ear being irrigated twice or more times daily according to the amount of discharge. Where early incision has been employed the discharge seldom lasts more than a few days. Where spontaneous rupture has taken place the discharge disappears only after a considerable period and is much more profuse. In the catarrhal cases cleanliness is of the utmost importance in order to prevent infection of the discharge in the canal and the subsequent development of a purulent inflammation. It is also necessary in cases where an incision is to be made to thoroughly sterilize the canal before operating, in order to prevent infection. This is best done by wiping out the meatus with an alcoholic solution of the bicholoride of mercury (1-3000). All instruments should be sterilized by boiling. Too much cannot be said in favor of an early free incision in all cases when the tympanic vault is involved, and in cases of acute catarrhal otitis which threaten to perforate spontaneously. Evacuation of the fluid by surgical means is always to be preferred to evacuation by spontaneous perforation.

Where the mastoid is involved, the early application of cold by means of the aural icebag, or the Leiter coil, may abort the inflammation. In regard to the particular part of the mastoid affected in these cases Politzer[1] has observed that the superficial cells are more frequently involved, while the antrum escapes, being shut off from the purulent focus by the swelling of the mucous membrane; he therefore deems it unnecessary in operating upon such cases to

[1]Archiv. für Ohrenheilkunde, Vol. XXX, p. 252.

enter the antrum and limits his operative interference to the superficial cells. In cases which have come under my own observation where the mastoid has been involved, the process has not limited itself to the superficial cells and I have found it necessary to perform the ordinary radical operation. There is no harm in opening the antrum and if it is not done a purulent focus may be overlooked. From the extreme rapidity with which the mastoid inflammation progresses early operation in these cases is to be advised. If cold is used, in the hope of aborting the inflammation, it should not be continued longer than forty-eight hours. It is true that the tenderness may disappear temporarily after the local application of cold for a longer period than that mentioned, but it has always been my experience, that operative interference was required at a later period, if cold applications had failed to obliterate local tenderness in forty-eight hours. The prolonged application of the ice-coil simply holds the mastoid inflammation in check, but does not cause it to disappear.

In cases demanding operation, either on account of the attempt to abort the mastoid inflammation having been unsuccessful, or because the patient was seen after the osseous tissue was so involved as to render all other treatment useless, the writer has been impressed with the amount of destruction which had taken place even in acute cases. Often the entire mastoid has been found to be filled with granulation tissue, although the disease was of but short duration. For this reason therefore a complete and thorough operation is always advisable, the entire mastoid process being converted into one large cavity by the free use of the curette and chisel, and free communication being established between this cavity and the middle ear in all cases.

The involvement of the perceptive apparatus as the result of epidemic influenza is much less common than involvement of the middle ear. It is sometimes impossi-

ble to determine whether the lesion is located in the nerve-trunk or in the labyrinth. A diagnosis of the involvement of the perceptive tract is made chiefly by exclusion, the cases not presenting upon functional examination the characteristic evidences of an affection of the conducting mechanism. Functional examination where the middle ear is involved, shows an increase in bone conduction, an elevation of the lower tone limit, that is, an impaired perception for tuning-forks of low pitch, while the perception for the highest sounds of the musical register is unimpaired. We occasionally find in cases where the hearing has become impaired after an attack of influenza that these symptoms are wanting, and that there is no evidence of any inflammation present or previous, in the Eustachian tube, or middle ear, to explain the condition. We find that the lowest notes of the musical scale are heard perfectly, that bone conduction is diminished, and that the upper notes are usually perceived poorly, although the upper-tone limit may be normal. Here we are compelled to believe that the labyrinth, or in some cases the auditory nerve-trunk, is the part affected. The changes which have taken place can only be surmised, but when we remember the circulatory disturbance which epidemic influenza causes it will not seem strange that the labyrinthine circulation should suffer. It seems probable that in many cases these circulatory disturbances may be angioneurotic in character.

The result of treatment in some of these cases would indicate that there had been an effusion into the bony labyrinth, increasing the labyrinthine pressure, and thus impairing audition. In one instance—that of a young man aged fifteen years—there was tinnitus and marked impairment of hearing. The audition was greatly interfered with by loud noises, the patient hearing better in a quiet place than in a noise. Examination showed a great diminution in bone conduction, while the low notes of the musical

scale were heard perfectly. Inspection and auscultation gave negative results. Under the administration of pilocarpine, and later of strychnia, all these symptoms disappeared completely and the hearing became normal. In some of these cases the presence of vertigo aids us in the diagnosis, while in others we must depend entirely upon the functional examination and the absence of any physical evidence of inflammation of the middle ear, in order to decide upon the location of the lesion.

There is one condition which deserves attention, and that is the ease with which the auditory apparatus is fatigued in certain cases of aural involvement after influenza. The hearing may be surprisingly good at the beginning of the examination, but as this is prolonged the power of audition gradually diminishes. These patients also complain that the effort to hear for any length of time causes great fatigue. This auditory fatigue not infrequently complicates an affection of the middle ear, and unless recognized, efforts at treatment are disappointing; the middle ear condition may yield entirely to treatment, and yet the power of audition will remain far below the normal standard. The use of strychnia in large doses seems to be the most successful means of overcoming this phenomenon. It is scarcely probable that this symptom depends upon any labyrinthine disturbance, and it is more than likely that it is due to certain changes in the auditory nerve-trunk. Gradnigo[1] reports an instance in which in addition to the fact that the auditory nerve became easily fatigued, there was also faulty perception for the middle notes of the musical register, a condition which he considers indicative of involvement of the nerve-trunk.

[1]Archiv. für Ohrenheilkunde, Vol. XXVI, p. 141.

DISCUSSION.

DR. C. S. BULL.—In discussing the localized complications of any general systemic disease, and especially of any infectious disease, we should beware of regarding all that follows a disease as its direct effect. This is particularly true of the complex of symptoms which we call influenza, and the French "*la grippe.*" Many of those attacked and weakened by influenza become, in consequence of this weakness, the victims of diseases which have no connection with the original cause of the weakness. The more closely we observe this disorder, the more convinced we become that the sequelæ of influenza bear no relation to the severity of the primary disease. Another fact which has been made clear by the study and observations of cases of influenza is that the general number of eye diseases is not increased during the prevailing epidemic.

So far as we know there is no ocular complication or sequela of influenza which is at all characteristic of the disease. Affections of the eyes in influenza are relatively not infrequent. They are polymorphous, of multiple variety, and many of them slight in nature and easily relieved.

I think that it may be stated positively that no one specific infecting agent has been with certainty found in all cases of influenza. Among the bacteria found in the circulation, secretions, or tissues of patients affected with influenza may be mentioned three varieties which are important from their consequences, viz.: The diplococcus, streptococcus, and staphylococcus pyogenes. All these may induce embolic and thrombotic processes in any part of the body, and also give rise to true products which poison all the functions.

It scarcely seems necessary to call in question the credibility of the belief that purulent processes occurring in the eye are dependent upon the toxic products of influenza. The question, very naturally, however, arises: Is the connection between the diseases of the eye and influenza direct or indirect, primary or secondary. Do toxic influences play the main rôle or do they merely lower the general health and thus pave the way for eye troubles?

The connection probably varies with different forms of eye disease. We start with the generally recognized as-

sertion that influenza is an infectious disease in which capillary thromboses have often been found. Emboli have been met with which are either infectious, or consist of simple clots. If they are infectious the streptococcus or the diplococcus or the staphylococcus pyogenes will be found. Many such cases have been reported in detail by the most careful observers, and there seems to be no reasonable doubt that purulent processes in the eye, the result in most cases of infectious embolism or thrombosis, are by no means infrequent complications or sequelæ of influenza. Of these the most important and the most disastrous in its consequences is the septic choroiditis or irido-choroiditis.

The most frequent purulent processes in the eye which accompany or follow influenza seem to be entirely external and are located in the lids, conjunctiva, or cornea. In these cases Jaccourd has advanced the theory that the infection is intrinsic,—that is, the numerous microbes of the conjunctiva, inoffensive when the patient is in good health, become noxious by the invasion of influenza. Fage[1] has reported cases of purulent dachryocystitis and abscess of the cornea with hypopyon. Landolt[2] has reported a case of abscess of the lid. Hosch[3] reports a case of purulent iridochroiditis with hypopyon. Eversbusch[4] reports in detail an extremely interesting case of purulent choroiditis occurring on the ninth day of the influenza and ending in panophthalmitis and perforation. The staphylococcus pyogenes aureus was found in large numbers in the secretion and infectious emboli were found in several of the blood vessels of the uveal tract. Fuchs[5] reports a case of purulent inflammation of the capsule of Tenon, beginning on the fourth day of the influenza with exophthalmos, loss of motility, swollen lids, and eventually a discharge of pus by two openings into the orbital tissue, and the pus showed pneumococci. Gazis[6] reports a similar case. Ehrlich[7] reports a case of abscess of Tenon's capsule, one of puru-

[1] Archiv. d' Ophtal., 1890, p. 316.
[2] Rec. d' Ophtal., 1890.
[3] Correspondenzbl f. Schweizer Aerzte, 1890.
[4] Münch. Med. Woch., 1890.
[5] Wien. Kl. Woch., 1890.
[6] Rec. d' Ophtal., 1890.
[7] Inaug. Diss. Berlin, 1892.

lent orbital cellulitis, one of abscess of the lid, and one of purulent dacryocystitis. Laquer[1] describes a case of bilateral embolic iridocyclitis which came on suddenly on the eighth day of the disease and ended in complete blindness of both eyes which lasted four days. The patient subsequently regained a vision of one half. Natauson[2] reports a case of influenza with pleuro-pneumonia and bilateral embolic iridochoroiditis ending in total blindness on the third day, and death on the fifth day. Finally in the monograph by Leyden and Guttmann on the influenza epidemic of 1889 and 1890, Horstmann, who wrote the chapter on affections of the eye, refers to cases by Pflueger, Greeff, Lyder, Borthem, and himself, showing the great variety of suppurative processes in the eye met with in patients sick with influenza.

Among a number of cases of purulent diseases of the eye in patients suffering from influenza during the last three years which I have seen, the following recent case seems worth reporting in detail, on account of its severity and fatal termination.

On Jan. 28, 1895, a woman, 40 years of age, was brought to me suffering from a very severe type of orbital cellulitis and panophthalmitis, and she gave the following history: The right eye had been blind for many years from a traumatism. On Jan. 23, she was attacked by many of the symptoms of influenza, coryza, obstinate cough, severe headache, high fever, pain in all her bones, and on the next day the eyelids began to swell and the eye to protrude. These symptoms rapidly grew worse, and when I saw her on the fifth day the eyelids were enormously swollen, the exophthalmos was extreme, the eyeball was very hard and immovable, and the pain was intense. Her temperature was 103 2-5°; her cough was violent and the expectoration profuse. She was put to bed; hot bichlorid fomentations were applied; leeches to the temple were ordered, and phenacetin and morphia were given to take down her temperature and allay the pain. The applications were continued for three weeks before the signs of cellulitis and panophthalmitis entirely subsided. As soon as softening began, two incisions were made into the orbital tissue on

[1] Berl. Klin. Woch., 1890.

[2] Petersb. Med. Woch., 1890.

both sides of the eyeball, and the pus then discharged freely. The cornea had previously ruptured, and a large amount of detritus and pus was evacuated from the eyeball. After all signs of cellulitis had subsided and pus had ceased to be discharged from the orbital tissue, the eyeball was carefully enucleated, and a clean, apparently healthy cavity was left. On the evening of the second day after the enucleation, the wound appearing perfectly healthy, she began to develop symptoms of meningitis, and she died comatose on the fourth day. The autopsy showed purulent meningitis which involved not only the whole base of the brain, but also the entire convexity as well. The extensive character of the meningeal inflammation led me almost to suspect that the meningitis had begun before the enucleation was done. At the autopsy no pus was found anywhere in the sheath of the optic nerve, but a subsequent microscopic examination may give a different finding. There seems scarcely any doubt that this was an embolic process,—whether starting in the cellular tissue of the orbit or in the uveal tract, it would seem impossible to say.

In regard to the affections of the nervous apparatus of the eye which are said to complicate or follow the influenza, including the muscles, the retina, and the optic nerve, the cases reported by competent observers are even more numerous than the cases of suppurative disease, and their actual connection with the general disease cannot be denied. The most frequent of these nervous sequelæ is probably a paralysis of accommodation. Here the toxic agent of influenza resembles that of diphtheria. Then we meet, in the order of frequency, with paralysis of the sixth or abducens nerve; paralysis of the motor oculi or third nerve, and paralysis of the trochlearis or fourth nerve. The muscles supplied by the third, fourth, or sixth nerves may be individually affected, or there may be complete external and internal ophthalmoplegia, either unilateral or bilateral. The paralysis of accommodation may occur alone without paralysis of the sphincter iridis, and in this peculiar effect the influenza resembles diphtheria. Three such instances have come under my own notice, and all regained the lost power of accommodation.

Schirmer[1] reports a case of right unilateral complete

[1] Klin. Monatsbl. f. Augenheil., August, 1890.

ophthalmoplegia, coming on during the height of an attack of influenza. There was loss of sensibility on the right side of the face and scalp, with paresis of the right masseter and temporalis muscles, and also of the muscles of the tongue on the same side. As there was paralysis of the third, fourth, fifth, sixth, and seventh nerves, the lesion must have been central, and was possibly a hemorrhage at the base of the skull, in or near the middle fossa. It is a well-known fact that in many cases of influenza there is a strong tendency to hemorrhage in various regions of the body.

Affections of the retina and optic nerve have been found to be almost as frequent complications of influenza as are paralysis of the ocular muscles, but they seem to be divided into two classes; one which accompanies the attack of influenza, and another which is a pure sequela, coming on several weeks after the onset of the disease. It is more obstinate in resisting treatment and leaves more disastrous results as to vision than the first class, because there is not only inflammation of the sheath of the optic nerve, but also acute degeneration of the nerve fibers. The papillitis or retrobulbar neuritis met with during or after the attack of influenza resembles the same conditions met with in the course of other infectious diseases. Two interesting cases are reported by Landsberg.[1] The first was a man, who, on the fourteenth day of the influenza, suddenly lost the sight of his left eye, without any ophthalmoscopic evidence of disease. He could count fingers at two feet, had a very narrow field with total color-blindness, and regained his vision after eleven daily injections of pilocarpin. The other case was a woman, who, during convalescence from influenza, had a sudden failure of vision of the left eye, with a central scotoma, proving the presence of a neuritis of the macular fibers, as the ophthalmoscopic examination was negative. She recovered under the administration of strychnia.

Ehrlich[2] reports cases of retrobulbar neuritis and papillitis, and Antonelli[3] the same. Snell[4] describes cases of optic neuritis ending in atrophy. Hartridge[5] reports a

[1] Centralbl. f. prakt. Augenheil., 1890.

[2] Inaug. Diss., Berlin, 1892.

[3] Annali di Ottal, XXI., p. 119.

[4] Trans. Ophthal. Soc. United Kingdom, 1892.

[5] *Idem*, 1892.

case of bilateral neuro-retinitis, and Metoxas[1] a case of retrobulbar neuritis, with hemeralopia, ending in atrophy.

Remak[2] reports a case of a man, with a severe catarrhal form of influenza, who on the fourth day had a series of four convulsions within one hour, followed by loss of vision and atrophy of the optic nerves.

Bergmeister reports cases of simple and inflammatory atrophy of the optic nerve, with concentric limitation of the field of vision and loss of the color sense.

Graddy[3] reports an interesting case of a woman who had a severe attack of influenza with pulmonary complications, and in the third week became suddenly blind, with a very small area of central vision for light. From this condition there was a slow improvement, followed again on the twelfth day by sudden failure of the vision, and right bilateral hemianopsia, which remained permanently.

Gifford[4] reports the case of a man, who, during a severe attack of influenza, suddenly had an onset of violent pain in the occipital region, followed by bilateral right-sided hemianopsia. In each eye the line of demarcation followed the vertical meridian accurately and remained unchanged for nearly three years.

The writer has recently had under his care the following case: A gentleman, 62 years of age, a literary man, badly nourished, and a victim for many years to intestinal catarrh, on the fourth day of a severe attack of influenza, suddenly noticed an obscuration of the vision of the right eye. I saw him within a few hours of the occurrence, found a central scotoma, positive in character, with a diameter of about 30° in all directions from the center of the field. Within the limits of the scotoma vision was absolutely lost. The ophthalmoscopic examination was entirely negative and has remained so. The scotoma for form gradually grew smaller and eventually disappeared, but the sense for color is still absent within the limits of the original scotoma. There was no pain caused by movements of the eye in the orbit, but considerable pain was produced by pressing the eye backwards into the or-

[1] Annales d' Oc., May, 1892.
[2] Centralbl. f. prakt., August, 1890.
[3] Ophth. Rec., June, 1892.
[4] Ophth. Rec., 1892.

bit. He recovered under the use of strychnia and pilocarpin administered hypodermatically. This was undoubtedly a case of retrobulbar neuritis, the fibers going to the region of the macula being involved.

DR. J. H. CLAIBORNE.—So far as my experience goes, there is only one disease of the eye which can be fairly attributed to the grip, and that is, catarrhal conjunctivitis. I have been very much edified by the two papers we have listened to, and I was greatly pleased by the conclusions given in Dr. Pooley's paper, which were conservative and distinctly in keeping with his scientific acumen. I fully agree with Dr. Bull that we should exercise the greatest amount of care before drawing any deductions in these cases. Every possible cause should be eliminated before attributing a certain condition of the eye to the grip. In his paper he cited many cases in which the evidence was conclusive and without any question. I was, for example, not certain until to-night that metastatic choroiditis had been observed in connection with grip; now I am convinced that it is so.

I have now under my observation a lady who a year ago had glaucoma. She was operated on by myself and made a good recovery, but a scotoma remained, which disappeared within the course of six or eight months. A few weeks ago she had an attack of grip and the scotoma returned.

I think that too much stress cannot be laid on the importance of recognizing the sequences and consequences of a disease, and although I have never seen these remarkable results following grip that have been reported here to-night, I am now fully convinced that they do occur, and I attribute the lack of knowledge on my part to want of experience rather than to want of observation.

DR. J. E. WEEKS.—On the 18th of May, 1891, I read a paper before the Section in Ophthalmology and Otology entitled "La Grippe as a Cause of Retro-Bulbar Neuritis and other Ocular Nerve Lesions," which was published in the *New York Medical Journal* for August of that year. The idea of the paper was suggested by a case of retro-bulbar neuritis occurring in my private practice, and in addition to this one, I collected fourteen others that had been reported by various observers. Of these

fifteen cases of neuritis of the optic nerve occurring in grip, nine were in females and six in males, the ages ranging from eighteen to fifty-eight years. There was an inflammatory condition at the disk in four cases; paling of the disk, more or less marked, in eleven cases. Blindness (permanent) resulted in one eye in one case, perception of light in two eyes in two cases. Approximately, complete recovery occurred in one case only. The scotomata produced were very varied, affecting all parts of the visual fields. Central scotoma without appreciable limitation of the visual fields occurred in two cases. The scotomata were for all colors excepting in the first case by D'Eperon, in which there was a central scotoma for red and green, and also a concentric limitation of the fields.

The conclusions expressed in my paper were as follows:

1. Neuritis of the optic nerve due to la grippe is of relatively rare occurrence; it may affect one or both eyes and may produce partial transient impairment of vision, partial permanent impairment of vision, or absolute permanent blindness.

2. Failure of vision begins from three to fourteen days after the commencement of the attack of la grippe, and proceeds quite rapidly. It is always preceded by intense frontal or circumorbital cephalalgia.

3. The form of scotoma produced is probably dependent on the position of the neuritis in the course of the nerve from the globe to the chiasm. If immediately behind the globe, the macular fibers are affected; if near the optic foramen the peripheral fibers suffer first.

4. Treatment has but little effect to promote a cure. If recovery follows, it takes place spontaneously and accompanies improvement in the patient's general condition.

5. The neurites of motor nerve branches resemble those that occur after diphtheria, and are mostly of a transient character. They may occur in any or all of the nerve trunks pertaining to the eye.

In the discussion which followed the reading of my paper at that meeting, the gentlemen who did me the honor to take part in it apparently had seen no such cases, and I suffered the consequences of advancing a relatively new opinion. Dr. Pooley at that time made some sweeping statements, denying the occurrence of such nervous

affections of the eye in connection with grip, an error which he has gracefully acknowledged this evening.

The following four cases of affections of the eye following grip have recently come under my observation: in these, as in the case of retro-bulbar neuritis reported in 1891, all other possible causes were excluded. The first case was one of paralysis of the external rectus of the right eye. The second case was one of neuro-retinitis hemorrhagica. The third case was one of papillitis affecting both eyes; failure of vision came on about a week after the attack of grip. The fourth case was one of neuritis of the supra- and infra-trochlear nerves.

Dr. H. S. Oppenheimer.—This subject has been so thoroughly discussed that I have very little to add to it. There is only one fact I wish to point out, and that is, that while the ocular lesions in grip *may be* in either one or both eyes, in the larger number of cases *they are* unilateral. This has been strengthened by the cases reported this evening, with the exception of the instances of paralysis of accommodation which is usually bilateral. The three cases coming under my observation, which were referred to by Dr. Pooley, have turned out very peculiarly. The case of retro-bulbar neuritis has not improved a particle. In the other cases the main symptom seems to have been hemorrhage. The first patient, the young man who had ecchymosis of the conjunctiva and lids, probably also had a slight hemorrhage into the sheath of the optic nerve. In the last case reported there were great splashes of hemorrhage between the nerve and the macule, with secondary exudation, so while there was actually some papillitis present, they should be considered, in my opinion, as the results of the hemorrhages. These two cases have improved gradually.

Dr. W. B. Marple.—I am in the same position as Dr. Claiborne as regards my experience with these cases. The only case of ocular trouble I have seen for which the grip could be fairly held responsible was one of paralysis of accommodation, from which the patient has not yet recovered.

Dr. Pooley (closing the discussion.)—I desire to express my thanks to Dr. Bull, who has supplied the few missing links in my paper. The cases reported by him in

detail are a very valuable addition to this subject. I was also pleased to learn that he was able to substantiate my position that inflammation of the uveal tract in these cases is due to metastasis. I think we may now fairly draw the conclusion that very important eye affections are dependent on the grip.

Stated Meeting, April 4th, 1895.

EXPERIENCES IN THE PRODUCTION AND USE OF DIPHTHERIA ANTITOXIN.

By HERMAN M. BIGGS, M. D.

The principles underlying the blood-serum therapy had been fully established so long ago as 1892 through the researches of Behring, Kitasato, Ehrlich, Roux, and others, but they were not reduced to a practical form, so that the method was available for the treatment of disease until 1894.

During the interval, Behring, Ehrlich, Roux, and others, together with Aronson, were diligently seeking to make this method available for the treatment of diphtheria.

Early in 1894, a small quantity of antitoxin produced by Aronson was placed at the disposal of the Health Department for experimental purposes, but the observations made with it were not very satisfactory.

In June, 1894, it was my privilege to watch the work being done in the Institute for Infectious Diseases in Berlin, to follow carefully the production of antitoxin there, and to obtain from conversation the personal impressions as to its value of the numerous staff of distinguished medical men connected with the Institute.

I found then that there was no shadow of doubt in the mind of any of these observers as to the specific value of antitoxin in the treatment of diphtheria. At this time about four hundred cases had been treated with serum produced in the Institute under the direction of Behring, Ehrlich, Wasserman, and Kossel. Exceedingly favorable

results had been obtained in this series of cases. Other cases in Berlin had been treated by Aronson, or with the serum produced by Aronson, and a still larger number by Roux in Paris.

At this time, and for some months later, very wide differences of opinion existed between Behring, Aronson, and Roux, as to the best methods for the production of immunity in animals and as to the kind of animals best suited for the purpose. While horses, cows, sheep, goats, and dogs were all being employed at the Institute for Infectious Diseases, still the belief at that time was that goats possessed many advantages over other species of animals. Behring believed that the more susceptible any species of animals was to the poison of diphtheria, the more highly could they be immunized and the more valuable was the serum which they produced.

Goats are extremely susceptible to the diphtheria poison, are difficult to immunize, but, when thoroughly immunized, produce a serum of high grade.

The remarkable results which Roux had obtained were not generally known until after the International Congress held in Buda-Pesth in September, and it was not until after the contents of his paper were widely published that the new treatment of diphtheria began to attract almost universal attention.

During my stay in Berlin, I requested by letter Dr. William H. Park, assistant director of the Bacteriological Laboratory of the Health Department, to commence the growth of cultures of the diphtheria bacillus for the production of toxins, as at that time it was thought necessary to grow cultures for six or eight weeks, in order to produce toxins active enough for the inoculations. This was done, so that, upon my return in August, the process of immunization of animals might be immediately begun.

No little difficulty was experienced in obtaining sufficiently virulent cultures of the diphtheria bacillus, and still

more difficulty was found in obtaining solutions of the toxins of adequate strength, so that finally the inoculation of animals under the supervision of the Health Department was not begun until early in October, 1894.

At first two goats, and then two sheep, three dogs, and a cow were inoculated, and some days later the inoculation of horses was begun. The number of horses was rapidly increased, and by the middle of December thirty-five or forty horses were under treatment.

After the publication of Roux's paper, such favorable results were reported as having been obtained with horses, that the inoculation of the other animals was soon suspended.

During all the time that the work has been done by the Health Department, Dr. T. Mitchell Prudden, consulting physician to the Department in the Division of Pathology, Bacteriology, and Disinfection, has been in constant communication with the physicians engaged in the work, and has assisted it in every way. A number of horses have also been treated under his personal direction by Dr. C. B. Fitzpatrick, and it was one of these horses whose serum was first tested as to its antitoxin value. The serum proved to have an immunizing power of something more than one in fifty thousand, and as the animal was otherwise in rather a poor physical condition, Dr. Prudden determined to destroy it in order that as large an amount as possible of serum might be immediately at disposal, as at that time the imported serum could be had only in the very smallest quantity.

Serum from this horse was tested and bottled to the amount of four litres or more, and was very generously placed at the disposal of the Health Department by Dr. Prudden, and employed under the supervision of the Health Department for the treatment of diphtheria both in the Willard Parker Hospital and among the poor outside the hospital. In behalf of the Department, I desire to

express the very great obligations which the Department and its representatives owe to Dr. Prudden and to the Pathological Department of Columbia College, of which he is the director, for the great assistance which has been afforded in this work.

The serum from other horses was not tested for several weeks after the first one, so large an amount having been obtained from it. As the treatment had been continued for so short a period it was deemed wiser, as long as this serum was at the disposal of the Department, to postpone the testing of other horses until it had been exhausted.

The first serum was put in use about January 1st, and since that time the amount of serum at our disposal has rapidly increased in quantity and improved in quality.

The assistance afforded by the Executive Committee of the *New York Herald* Antitoxin Fund has very greatly facilitated the work of the Health Department, and has enabled it to produce the serum in larger quantity and more rapidly than would otherwise have been possible.

The cases of diphtheria treated with antitoxin under the supervision of the Health Department comprise two series: the first, those treated in the Willard Parker Hospital, and the second, those treated outside of the hospital, chiefly among the poor in the tenement-house districts, by Health Department inspectors, detailed from the division of Bacteriology for this purpose.

The second series includes those cases treated by Sanitary Inspectors Drs. Walter F. Bensel, John H. Huddleston, Henry F. Koester, H. J. Ennis, William J. Pulley, and a few which were treated by Dr. William H. Park and the writer. There were 255 of these cases, with 40 deaths, a mortality of 15.69 per cent. Of these, 50 are recorded as having been mild, 76 of moderate severity, 96 severe, and 42 severe and septic. Of 252 cases noted, in 21 the larynx alone was involved; in 116 the pharynx, or tonsils, or both, were involved; in 57 the pharynx and

nares were involved; in 41 the larynx and pharynx were involved, and in 17 the larynx, pharynx, and nares were all involved. One hundred and seventy-three cases were five years of age or under, and among these 33 deaths occurred, a mortality of 19.07 per cent.; 82 cases were over five years of age, and among these 7 deaths occurred, a mortality of 8.53 per cent. On the first day of the disease 30 cases were treated; deaths, 2; mortality, 6.66 per cent. On the second day 77 cases were treated; deaths, 7; mortality, 9.09 per cent. On the third day 67 cases were treated; deaths, 11; mortality, 16.41 per cent. On the fourth day 31 cases were treated; deaths, 7; mortality, 22.60 per cent. On the fifth day, or later, 46 cases were treated; deaths, 13; mortality, 28.25 per cent.

Of the 40 deaths occurring in the whole series, 15 took place in less than twelve hours after the serum was administered. These cases were hopeless, and most of them were moribund at the time of the injection. They were injected by the inspectors, for the most part under protest, because the physicians in attendance or the friends of the patients insisted that antitoxin should be administered. These may fairly be excluded from consideration in judging of the results of the new treatment, as death occurred before sufficient time had elapsed for the serum to produce its effect. Deducting them, we have 240 cases with 25 deaths, or a mortality of 10.4 per cent.

Among the cases treated on the first day of the disease two deaths occurred. The first death occurred in a child a little over two years old, suffering with very severe laryngeal diphtheria. This case was injected and intubated within the first twenty-four hours of the illness; death was only delayed by the administration of the antitoxin, and took place on the tenth day from broncho-pneumonia. Dr. Bensel, who treated this case, says: "I feel absolutely certain that death would have taken place in

this case within the first forty-eight or seventy-two hours of the disease if antitoxin had not been administered."

The second death was also the result of severe laryngeal diphtheria with stenosis; it occurred in a child thirteen months old. The child was intubated, and died of stenosis on the third day after the removal of the tube.

Of 77 cases treated on the second day, 7 died, a mortality of 9.09 per cent. Three of the fatal cases were moribund at the time of injection and died in less than twelve hours.

Deducting these, there remain 74 cases and 4 deaths, a mortality of 5.04 per cent.

Of 67 cases treated on the third day, 11 died, a mortality of 16.41 per cent. All of the fatal cases in this group, with one exception, had laryngeal diphtheria, or laryngeal and pharyngeal.

In 4 of the fatal cases the patients were moribund when antitoxin was injected; 1 died an hour after injection; 1 four hours after; 1 was complicated with scarlet fever and septic poisoning, was pulseless when injected and died a few hours later; and 1 was suffering from severe laryngeal stenosis and broncho-pneumonia, was not intubated, and died from unrelieved stenosis, with high temperature, a few hours after the injection.

In one case treatment was discontinued for four days because of the objection of friends, although decided improvement had followed the first injection. It was then resumed, but death from cardiac paralysis occurred later.

These 5 cases may be fairly excluded from consideration. We then have 62 cases treated on the third day, with 6 deaths, or a mortality of 9.68 per cent.

Of 31 cases treated on the fourth day 7 died, a mortality of 22.06 per cent. Six of the fatal cases suffered from laryngeal diphtheria, and one from naso-pharyngeal. Three of these were moribund when injected and died within less than twelve hours; one of the remaining seven

had been intubated; the child had greatly improved, and on the ninth day the tube was removed; the child died suddenly from stenosis one-half hour later, after the physician in attendance had left.

Excluding the 3 moribund cases from this group, there remain 28 cases, with 4 deaths, a mortality of 14.28 per cent.

It does not seem desirable that any special analysis should be made of the cases which were treated on the fifth day or later, as all of the evidence goes to show that, under most circumstances, only moderate benefit can at best be obtained from the use of antitoxin in such cases, and although, occasionally, striking results follow the administration of antitoxin, even late in the disease, yet this result must be considered as exceptional.

If we consider again the whole series, after deducting those which were treated on the fifth day or later, we have 209 cases, with 27 deaths. Of the 27 fatal cases death occurred in twelve hours from the time of injection in 12. Deducting these there remain 197 cases of diphtheria treated before the fifth day, with 15 deaths, a mortality of 7.79 per cent. Of the 197 cases in this group 126 were five years of age or under, and of these 11 died, a mortality of 8.72 per cent.

In judging of the results obtained in this series, it must be kept in mind that almost all occurred among very poor people in the tenement-house districts, where, in many instances, the care was not of the best, and where often but little in the way of other treatment was employed. With the exception of possibly a half dozen, the diagnosis was established in every case by the demonstration of the presence of the diphtheria bacilli in the throat, so that there can be no doubt as to the nature of the disease which was being treated.

Cases Treated in the Willard Parker Hospital.—I am indebted to Dr. W. L. Somerset for the data regarding

cases treated in the hospital. Far less favorable results were obtained in these cases than in those treated outside. Hospital cases are, in general, severe; usually do not come under observation until relatively late in the course of the disease, and often then are suffering from serious complications.

From January 1st to date there were 164 cases of diphtheria in the hospital treated with antitoxin, with 45 deaths, a mortality of 27.4 per cent. During the first three months of 1894, 146 cases were treated, with 47 deaths, a mortality of 32.1 per cent. During January, 1895, however, the antitoxin was weak. From February 1st serum equal to Behring's No. 1, or better, was at command. After this time 115 cases were treated, with 31 deaths, a mortality of 26.9 per cent. Excluding 2 cases that died within twelve hours after the administration of the serum, we have 113 cases, with 29 deaths, a mortality of 25.6 per cent.

Of the 115 cases, in 44 the larynx was involved, and in 71 the pharynx, or the pharynx and nares.

In the pharyngeal and nasal cases there were 10 deaths, or a mortality of 14.08 per cent. In 1894 (during the first quarter) there were 104 pharyngeal and nasal cases, with 25 deaths, or a mortality of 24.03 per cent. Of the 44 laryngeal cases 21 died, a mortality of 47.7 per cent. Twenty-eight of the 44 cases were intubated, and of these 16 died, or a mortality of 57.1 per cent. Excluding from these 28 cases 9 (of which 7 died) which were treated with antitoxin on the fifth day or later, there remain 19 cases of laryngeal diphtheria with intubation, and 9 deaths, or a mortality of 47.3 per cent.

In 1894 16 cases, *all included*, were intubated, with 13 deaths, a mortality of 81 per cent.

Sixteen of the laryngeal cases were not intubated, and of these 5 died. If we exclude from this class the cases treated on the fifth day or later, we have 11 cases, with

3 deaths, or a mortality of 27.27 per cent. In 1894, during the first three months, there were 26 laryngeal cases which were not intubated, and of these 9 died, a mortality of 34.61 per cent.

During the period referred to in 1895, all cases of *diphtheria* admitted to the hospital were treated with antitoxin, and are included in this list. Cases in which the diphtheria bacilli were not found are not included.

Notes on Some of the Fatal Cases.—Fatal cases injected on the third day of the disease:

CASE I.—Katie J——, two years of age, admitted with mild tonsillar and pharyngeal diphtheria and marked general bronchitis. Death occurred thirty days after admission from extensive broncho-pneumonia. The autopsy also showed general miliary tuberculosis.

CASE II.—Meyer A——, aged seventeen months, admitted with diphtheria bacilli, and no visible membrane in throat. Right eye showed membrane on lids and suspicious bacilli. Eye made good recovery. Died thirty-two days after admission with broncho-pneumonia. Child ran normal temperature two weeks previous to the onset of the pneumonia.

The autopsy showed lobar pneumonia involving the right upper and middle lobe.

Fatal cases injected on the third day of the disease:

CASE I.—S. D——, eighteen months of age; case admitted with extensive pulmonary œdema and marked stenosis; was then cyanotic, unconscious, and pulse could not be found. Intubated at once. Artificial respiration. Consciousness regained. Color improved, but the lungs continued full of fluid, and the child died in twenty-four hours. Membrane visible on left tonsil only.

CASE II.—Becky C——, two and one-half years of age, admitted with severe stenosis. She was immediately intubated, and did well for eight days, but then developed broncho-pneumonia, and died twelve days after admission. Autopsy showed extensive broncho-pneumonia.

CASE III.—Martin Jacob S——, two and one-half years of age, admitted with severe stenosis and considerable tonsillar and pharyngeal membrane. Nose partially ob-

structed. Cervical adenitis. Broncho-pneumonia. Lived forty-eight hours after admission. Was intubated and tracheotomized. Considerable membrane coughed up through tracheotomy tube.

CASE IV.—Sarah L——, two years of age, admitted with slight tonsillar deposit and slight stenosis, prolapse of the rectum, and marasmus. Probable broncho-pneumonia. Lived thirty-two days. Temperature 99° to 101° F.; respiration, 24 to 40. Coarse râles, roughened breathing sounds persisted over both sides of chest behind. Throat clear from membrane in three days. Croup did not persist.

CASE V.—Florence H——, eight months of age, admitted with mild tonsillar diphtheria and slight stenosis. Sixteen days after admission a cervical abscess was opened. Twenty-eight days after admission, there was a purulent discharge from both ears. Temperature went up after the ears began to discharge. The child became restless, cried constantly, and refused nourishment. Death occurred seven days after rupture of the otitis, thirty-two days after disappearance of membrane, and twenty days after disappearance of diphtheria bacilli from pharynx.

CASE VI.—Elsie G——, two years of age, admitted with well marked tonsillar and pharyngeal deposit and stenosis. Intubated; stenosis relieved; temperature, 106° F.; respiration, 50. No physical signs. Appearance of membrane in pharynx would indicate that case was more than three days sick. Died the third day.

Two other fatal cases died in less than twelve hours after the administration of antitoxin. These cases were moribund on admission.

One other case showed at the autopsy a general miliary tuberculosis, and three showed pulmonary tuberculosis with more or less broncho-pneumonia.

Two other cases had scarlet fever and died some time after removal to North Brothers Island.

In a number of other fatal cases death occurred some days after the disappearance of the membrane, and the autopsy revealed a broncho-pneumonia.

In speaking in general of the value and results of the

treatment I wish to refer to some extracts from a report made to me by Dr. Walter F. Bensel. He says:

"The total number of cases treated by me was 82. Of these 70 recovered, 12 died, making a mortality for all ages of about fifteen per cent. Sixty of these showed immediate and marked improvement after the administration of the serum. In 6 there was marked improvement, which was, however, developed very slowly. It may or may not have been due to the antitoxin. In the remaining 16 no benefit could be attributed to the antitoxin directly. In 34 of the 82 cases the prognosis was exceptionally bad, making an estimated probable mortality of 41 per cent. without antitoxin, which contrasts very favorably with the actual mortality of 15 per cent.

"In 12 cases treatment was begun on the first day, and all showed an immediate improvement. In 25 cases treatment was begun on the second day of the disease, and all but 3 were promptly benefited. In 17 cases treatment was begun on the third day, with an immediate result in 13. Of 11 treated for the first time on the fourth day only 7 were benefited. For the fifth day only 4 out of 8, for the sixth day only 1 out of 3, and for the seventh day only 1 out of 4 had an immediate benefit from the antitoxin. Of the 2 cases treated for the first time on the tenth day no direct result was obtained. This shows how important it is to employ antitoxin early, as few or no striking results occur when the fifth day of the disease has passed before beginning treatment.

"In my experience the effect of the antitoxin on the false membrane has always been very marked. Even when little or no improvement in the general condition has resulted, the advance of the membrane has been checked and a marked disposition to thin out and disappear has been developed.

"*After-effects of Antitoxin.*—In 82 cases of true diphtheria 33 remained absolutely free from any annoying after-effects. In 22 of the remainder urticaria was developed, varying in degree and extent. In some it was limited to the limb or limbs injected, and only caused slight annoyance for two or three days. In other cases it extended from head to foot, the irritation being very intense and lasting a week or more. It made its appearance from three to ten days after the injection.

"In 8 cases an erythema was developed, usually more or less confined to the locality of the injection, and making its appearance two or three days afterward. This was accompanied by more or less urticaria.

"One case of diphtheria (a rather mild form, but receiving a fairly large amount of serum in two doses) developed a general urticaria eight days after the second injection, the itching of which was almost unendurable. This lasted four days, but had hardly subsided before the patient, a woman, began to develop pains, neuralgic in character, in the lower limbs, with some slight swelling of both knees. This condition grew worse from day to day, and soon all the joints of the lower extremities were swollen, painful, and stiffened, with a very marked hyperæsthesia of the skin. Loss of power was complete, so that she was unable to walk or even to stand. On the third day she also began to lose power in her upper extremities, so that she was unable to move in bed without assistance. The last muscles affected were those of the jaw, preventing mastication. On the eighth day the patient had several profuse hemorrhages from the nose, which recurred, but not so profusely, from time to time during the next week. About this time she began to regain power in her limbs, and in a few days was able to stand and then to walk with difficulty. It is now thirty-eight days since her attack of diphtheria, and she is still unable to walk more than a few blocks at a time. The lymphatic glands in her neck were more swollen and tender twenty-six days after her recovery from diphtheria than at any other time. They are still much swollen, but no longer tender. A trace of albumin was found in the urine during the development of the complications.

"In 7 cases of false diphtheria, injected while awaiting the result of the culture, only 2 were exempt from after-effects. These after-effects were, in the main, much the same as in the cases of true diphtheria just mentioned, but were developed in a larger proportion, and were more marked in degree. One patient developed the same trouble as the case just described, but to a much more serious extent in every way. In addition she had several alarming hemorrhages from the uterus, and a temperature ranging from 100° to 104° F. Although the antitoxin was given on February 5th, she has not yet left her bed (March 10th). About two weeks ago she developed an acute syn-

ovitis of the knees, with fluid in both joints and in the pericardial and in one pleural cavity. I am hardly prepared to say, however, that all her trouble has come from the antitoxin."

It should be said that in these cases large doses of a weak serum were employed.

In fatal cases treated by antitoxin no new anatomical lesions have been found. There is no positive evidence as yet to show that antitoxin in any case has produced or hastened death, although it may apparently, in rare instances, produce serious complications.

It is, however, comparatively common to have characteristic eruptions follow the use of this remedy, and sometimes apparently more serious complications, as described. The frequency of the rashes varies greatly, but they appear to be more common when antitoxin is administered in fairly large doses in mild cases. The eruption may take the form of an urticaria, or may be scarlatiniform or morbiliform in character. It may be local around the point of injection or general. The time of appearance varies from five to twenty days after the injection of the serum, the average being about eight or ten days. It is usually accompanied by intense itching; there may or may not be elevation of temperature. Its duration varies from a few hours to four or five days or longer, but usually it disappears in about thirty-six or forty-eight hours.

In 129 cases treated in the Willard Parker Hospital, there were rashes in 20. In 12 cases the rash was an urticaria, in 5 it was like that found in scarlatina, and in 3 it resembled measles.

In 21 out of 129 cases there was albuminuria, and in 6 there was paralysis, slight in amount. Two of the cases of albuminuria died with suppression.

The serum employed in the treatment of the cases reported here, with the exception of a few phials of Behring's and that referred to, received from Dr. Prudden, was

produced under the supervision of the Health Department. It varied in strength from that first used, which contained 30 or 40 antitoxin normal units in each cubic centimetre, about one-half the strength of Behring's No. 1, to some of that last produced, which contained 300 antitoxin normal units in each cubic centimetre, equal to about twice the strength of Behring's No. 3.

The Health Department now has an abundance of high-grade serum, and is prepared to meet all demands arising from this city. It is administered under the supervision of the attending physician, free of charge, among the poor of the city on application. It is also for sale when the physician prefers to purchase it and administer it himself.

It should be remembered in judging of the results obtained in the treatment of cases of diphtheria with the diphtheria antitoxin, that the new remedy is a specific only against diphtheria toxæmia, and that it thereby increases cell-resistance, reduces inflammatory swelling, and arrests the extension of the membrane—only this. It does not restore already degenerated heart-fibres or kidney cells; it does not immediately relieve an impending asphyxiation from laryngeal stenosis, or neutralize a septic poisoning, or influence the temperature, or check the extension of a broncho-pneumonia. To be effective it must be administered early in the course of the disease, before complications have occurred or the effects of the diphtheria toxin on the tissue-cells have been produced. The earlier it is administered, the more striking are the results produced and the more certain the cure. When not administered until the fifth day of the disease or later, or until a general bronchitis or broncho-pneumonia has occurred, or degeneration of the heart or kidney has been produced, or until the child is exhausted from struggling against an unrelieved stenosis, its influence for good is little or nothing, and deaths in such cases are not to be regarded as antitoxin failures.

It may be urged that no conclusion can be drawn from these results as to the value of the new treatment because diphtheria is of a mild type this year. This contention cannot be maintained, for there is no reliable evidence to this effect, and before the introduction of antitoxin in the treatment of diphtheria the mortality during the past year was as high as for the corresponding months of the previous year. During the same period, when these cases were being treated, the mortality among cases not treated with antitoxin, instead of averaging eight or ten per cent., was, judging from the health department reports, from twenty-five to thirty-five per cent.

While there can be no question that occasionally cases of uncomplicated diphtheria will die even when antitoxin is administered early in the course of the disease, yet in my opinion, with the curative serum at disposal, diphtheria, which has been the most fatal disease of infancy and early childhood, will be brought so completely under control, through the early administration of this remedy, that it will lose all of its terrors. How serious the complications may sometimes be we do not as yet know.

Statistics may or may not have much significance, and it may perhaps be justly said that, so far as New York is concerned, nothing decisive has as yet been shown by them as to the curative value of diphtheria antitoxin. I would add, however, that the results in individual cases are often far more convincing than the conclusions drawn from a large series of cases. All that is required to convince any sceptical observer of the efficacy of the serum is that he may watch the results in one single, severe, uncomplicated case of diphtheria, when the remedy is administered on the second or the beginning of the third day. They are sometimes most extraordinary, and seem to me to approach the miraculous more nearly than anything which has previously come under my observation in medicine.

THE PREPARATION OF THE DIPHTHERIA ANTITOXIN,

AND SOME OF THE PRACTICAL LESSONS LEARNED FROM THE ANIMAL EXPERIMENTS PERFORMED IN TESTING ITS VALUE.

BY WILLIAM HALLOCK PARK, M. D.

The first step toward obtaining a supply of diphtheria toxin for the injection of the animals is to secure pure cultures of bacilli from a number of severe cases of diphtheria. The bacilli from these cultures are grown in bouillon for forty-eight hours in the incubator, and their virulence tested by injecting them into guinea-pigs. From the number tested as many as four or five of the most virulent cultures should be selected to inoculate the bouillon for making the toxin, for experience has taught that bacilli having equal virulence in guinea-pigs do not always produce the same amounts of toxin in bouillon. It has also been found that the bacilli in cultures from different cases vary greatly in the length of time the bacilli retain their full virulence. In making the original stock-cultures it is desirable to inoculate the bouillon in each of the tubes with bacilli from a number of colonies from the agar plates. The cultures thus obtained are more apt to be very virulent. The experiment of trying to increase the toxin-producing power of the bacilli by passing them through the bodies of a series of guinea-pigs, has been carefully tried in the laboratory. Dr. Williams, who carried out the experiments, succeeded in considerably increasing the virulence of the bacilli from some of the cultures, and obtained in the broth in some of the tubes inoculated with these bacilli, a stronger toxin than that from the broth inoculated with bacilli which had

not passed through the bodies of animals. However, the strength of toxin varies so much in the bouillon in different flasks, even when made from the same culture, that we do not feel sure that the increased toxin production of the bacilli was due to this passage through the animals. The practice recommended by Roux, of inoculating the bouillon in each flask with a number of cubic centimetres of a recent and very virulent culture, has been followed as a routine practice. The flasks used for the preparation of toxin may be either of the ordinary varieties or may be arranged with tubes for the inlet and exit of air. We have found that bacilli grown in rather alkaline bouillon, in ordinary flasks loosely plugged with cotton, produce toxins about as strong, and nearly as quickly, as those grown in the bouillon to which a constant supply of moistened air has been added. Toxins, of which one-tenth of a cubic centimetre was sufficient to kill a 250-gm. guinea-pig in five days, were produced in the bouillon in one-half the flasks in from two to four weeks. In some cases the toxins were twice the above strength, while in others, subjected to the same treatment, they were much weaker. When it is judged that the bouillon culture is sufficiently rich in toxin, a few cubic centimetres are carefully removed with a sterilized pipette, and the toxin solution thoroughly tested as to its purity and strength. If the cultures prove in the least contaminated they are, as a rule, discarded, but if merely too weak in toxin they may be replaced in the incubator for further development. If to an old bouillon culture one-half per cent. of carbolic acid is added, and the bacilli, after being killed, are allowed to soak in the bouillon, the amount of toxin in solution will be found for a time to gradually increase. The dead bacilli give up some of the toxin contained in their bodies to the bouillon. Before being injected into the horses the toxins are filtered.

Selection of the Horses.—For this purpose Dr. Gill, of

the New York Veterinary College, procured, during the months of November and December, over forty young, perfectly sound horses. Most of these had some bad habit, such as kicking, balking, or running away, and were, therefore, purchased at very low prices. Before using they were all injected with mallein in order to exclude the possibility of glanders.

Since the laboratory force of the Health Department was increased, last January, I have had the help of Drs. Lambert and Fitzpatrick in the labor connected with the production of antitoxin in so many horses. These horses were purchased partly by the Health Department and partly by means of the fund raised by the *New York Herald*. The horses receive for their first injection, 2 c.c. of a toxin of which one-half a cubic centimetre would kill a 250-gm. guinea-pig, or an equivalent amount of stronger toxin. If the horses reacted sharply with fever, extensive local swelling and loss of appetite, the dose was not repeated until all these symptoms subsided. If the horses showed no reaction, they were given, in two days, another dose of double the quantity. In the course of ten days the use of strong toxins was begun in all. Treated in this way, the majority of the horses will be able to take 100 c.c. of strong toxin in a single dose at the end of from two to four months without showing any marked disturbance. A few horses are very sensitive, and these are very difficult to accustom to the toxins. Among the forty injected seven have died.

At present there are ten horses containing sufficient antitoxin in their serum for the treatment of diphtheria, and a number giving serum of sufficient strength for purposes of immunization. One horse gives serum one-half stronger than Behring's No. 3; one gives serum equal to that of Behring's No. 3; two equal to that of Behring's No. 2, and five give serum equal to Behring's No. 1.

The Methods Employed in Bleeding the Horses.—The

skin of the neck having been shaved and cleansed, the jugular vein is pressed upon low in the neck and caused to distend. Over the vein the skin is incised, and through this incision a sharp-pointed cannula is forced into the vein. The stream of blood is conducted, by means of a rubber and glass tube, into sterilized Erlenmeyer flasks.

To me the great difference in the amount of antitoxin furnished by different horses which have received the same amount of toxin, and in the same doses, is of great interest. For instance, two horses started on November 4th, had received by January 15th 600 c.c. each of normal toxin; at that time the antitoxin strength of the serum of the first horse was equal to that of Behring's No. 2, while the serum of horse number two was only one-fifth of that strength. On March 1st the strength of the serum from the first horse had risen to double that of Behring's No. 2, while that from horse two remained only one-eighth of that strength. Both of these horses had shown a moderate elevation of temperature after each injection, and both had shown only slight general disturbances. The better horse was slightly more sensitive thau the other to the toxin. Both horses seemed to suffer but little under the treatment. An interesting experiment was now tried. As you know, several of the most valuable horses abroad died from attempting to procure a very high grade of serum by the injection of very large doses of toxin. We thought it best to let well enough alone, and so gave three of the best horses during the last month only very small doses of toxin. The tests just concluded show, unfortunately, that under this treatment the amount of antitoxin has begun to diminish, so that it seems here, as elsewhere, that nothing venture, nothing have.

The Testing of the Antitoxin.—At present we use entirely Behring's method. His method possesses certain great advantages over that of Roux. First, by using toxins instead of the bacilli, we have a substance which

remains of constant strength. As is well known, cultures of the living bacilli vary from day to day in their virulence. The bacilli contained in two tubes taken from the same culture may, after forty-eight hours, possess very different amounts of virulence. To accurately test by Roux's method, a large number of guinea-pigs must be used each time as controls, and even then the test is not as delicate as that of Behring's. We have found, on the other hand, that the diphtheria toxin, kept in small flasks at a rather low and even temperature, keeps for months of almost exactly the same strength. After a little while we are thus able to accurately determine the amount of toxin necessary to surely kill a guinea-pig of a given weight. In the course of a very large number of experiments in testing antitoxin, we have found that, whenever in a series the amounts of antitoxin injected have been too small to protect, the guinea-pig receiving the smallest amount of antitoxin has always been the first pig to die; the one receiving the next smallest amount being always the second to die. When the results obtained have been controlled by later trials, we have found them substantiated in every case.

The serum from each bleeding is always tested as to its strength in antitoxin. This is absolutely necessary, as in the course of two weeks the strength of the serum may either greatly increase or diminish, and for this change there may be no ascertainable reason.

Therapeutic Hints from the Animal Experiments.—If in a guinea-pig very virulent diphtheria bacilli are rubbed upon a portion of mucous membrane which has been excoriated or cauterized, a disease very similar to diphtheria in man is produced. For two or three days the local changes develop, and then later the constitutional ones. Death follows in from four to five days. If, however, a guinea-pig thus treated has within a short time previously been injected with diphtheria antitoxin, slight local

changes only develop, and the pig recovers. The same results follow if sufficient antitoxin is injected subcutaneously within the first twenty-four hours after the infection. If the antitoxin is given too late no beneficial results follow. For instance, each of ten 500-gm. guinea-pigs was injected with ten times a fatal dose of diphtheria toxin—a dose sufficient to kill within twenty-four hours. Two of them received $\frac{1}{1500}$ c.c. of antitoxin serum, an amount of antitoxin equal to one-fifth of an antitoxin unit; four others received, twelve hours after the toxin injection, respectively, seven, fifteen, and thirty times the quantity of antitoxin received by the first two; finally, twenty hours after the injection of the toxin two pigs received seven hundred, and one thousand five hundred times as much antitoxin as the two pigs first mentioned. The pigs receiving the minute dose of antitoxin at the same time as the toxin showed no bad symptoms whatever, while those receiving the very large doses twelve to twenty hours after the injection of the toxin all died. The large doses given twelve hours, and the immense doses given twenty hours after infection, and four hours before the expected death, produced no curative effect, the pigs dying at almost the same time with the pigs which received no antitoxin whatever.

Mixed Infection.—The experiments of Roux with mixed infection are so interesting in this connection that I need hardly apologize for giving them here in brief.

Four rabbits were tracheotomized and then injected in the trachea with a mixture of streptococci and diphtheria bacilli. One received at the same time a subcutaneous injection of antitoxin; the second, an injection of twice that quantity six hours later, and a third, five times the quantity twelve hours after the infection. The fourth received no antitoxin. The first rabbit was feverish and slightly sick for two days, no diphtheria developed, and the animal completely recovered. In the second, diphtheria

developed, progressed, and caused death in five days; in the third, the disease progressed just as rapidly as in the fourth, which had received no antitoxin. All the fatal cases showed at autopsy the lesions of broncho-pneumonia, and the cultures showed streptococci to be present in the blood.

From animal experiments, then, we have these facts: A small but sufficient amount of antitoxin will certainly prevent animals from developing the lesions of diphtheria after they have been infected with the diphtheria bacilli alone, or mixed with streptococci, if the serum is given before, at the time, or shortly after, the attempted infection. If, however, the injection of antitoxin is delayed too long, it will not have any appreciable effect in preventing the progress of the disease. A longer delay is possible when the infection is due to diphtheria bacilli alone than when the bacilli are combined with the streptococci. After several months the effects of the injection of the antitoxin pass away, and the animals are as susceptible as before they received the injection. Another injection gives them immunity again for a length of time.

From the results in animals we have a right to hope that antitoxin, injected into human beings, will protect them for days, and probably for weeks, absolutely from an attack of diphtheria. A larger amount of antitoxin should cure cases of diphtheria when the lesions are limited to superficial local changes in the pharynx or larynx. We should have less hope for any effect in cases where pneumonia or sepsis had developed, where other micro-organisms played an important rôle, or in cases treated within a few hours of probable death.

IMMUNIZATION AGAINST DIPHTHERIA WITH ANTITOXIN.

BY GEORGE A. PECK, M. D.

Instead of entering at once into giving the results obtained from the use of antitoxin as a means of preventing the development of diphtheria, it may, perhaps, be best to give a short history of the epidemic of diphtheria that has been holding sway in the Mount Vernon branch of the New York Infant Asylum for the past few months, in order that the conditions under which this remedy has been employed may be better understood and appreciated.

From May, 1892, to February 18th, 1894, there was no diphtheria in the asylum—a period of one year and nine months. On the latter date, however, the disease developed in a child received into the asylum but twelve days before from the New York department of the asylum; two weeks later a second case appeared, ten days after this a third, and so on, there being a mild epidemic of nine cases in the May following, the disease then becoming epidemic till September 5th, 1894, when the present epidemic may be said to have actually begun. During this interval, February to September, a period of but little more than half a year, there were 22 cases of diphtheria and 15 deaths—a mortality of 66 per cent. The diagnosis in these cases was made by clinical and macroscopic conditions combined, and could bacteriological examinations have been made in some instances, the number would have undoubtedly been increased, with diminished per cent. of mortality, since there were a number of cases of so-called tonsillitis during this period identical with cases that have since been proven to be true diphtheria.

On September 5th, 1894, two cases of diphtheria developed, followed by 14 other cases during that month. In October there were 32 cases; in November, 30 cases; in December 31, January 14, February 10, and March 3, giving up to date, 136 cases of diphtheria since September 5th, 1894, and 158 cases since February 18th, 1894.

During the early part of this epidemic the diagnosis of diphtheria was made from bacteriological examinations by the New York City Board of Health, but later it became necessary to establish a laboratory in the asylum for this particular department of bacteriology, by aid of which results could be quickly learned and a more extensive work could be carried on. In the latter part of October, when this was done, bacteriological examinations of the throats of healthy children revealed the fact that Loeffler bacilli were present in so many instances that, in order to isolate these cases, nearly half of the children of the institution were quarantined. Special attention was paid to these children with regard to checking the progress of the epidemic, but all attempts in this direction were futile.

Through the kindness of Drs. Biggs and Park of the New York City Board of Health, a quantity of Behring's antitoxin No. II was given for use in immunizing against diphtheria. Accordingly, on December 12th, forty-two children were selected whose throats at that time contained the Loeffler bacilli, and whose ages ranged from one and a half to five years. These children were placed in one of the buildings. To one half their number (twenty-one) was given the antitoxin, each receiving 150 units. No diphtheria developed among these children, including those who did not receive the antitoxin, for a period of one month,—at the end of that time diphtheria developed in a boy four and a half years old, while convalescing from a severe attack of pneumonia, he being one who had received 150 units of antitoxin.

In the meanwhile the epidemic was persisting, cases having developed in other portions of the institution, and the situation was becoming more serious and alarming each day. That the disease should have gained such a wide-spread hold cannot be wondered at when one considers the capacity of the wards, which ranges from twenty to thirty children; and since no building or ward has been exempt from the disease, almost every child has repeatedly been exposed to infection, not only from those developing the disease, but also from those healthy children who did not develop diphtheria, but whose throats contained the Loeffler bacilli.

Although the first experiment with antitoxin proved nothing, except that the period of immunity it rendered was limited, it was decided to inoculate with antitoxin all the children remaining in the institution who had not had diphtheria, as soon as the remedy could be obtained. Accordingly, on January 16th and 17th two hundred and twenty-four children were inoculated with Behring's antitoxin No. II, the quantity of antitoxin given in each varying from 100 to 200 units, according to age. An examination of the records shows that from January 4th to 14th, 1895, twelve cases of diphtheria developed, nine occurring in the last five days of this period. The first case to appear among these children who were inoculated was on February 3d, two and a half weeks after the antitoxin was administered. On February 15th a second case appeared among these children, the interval in this instance being four weeks from inoculation. Preparations were then begun for a second general inoculation, should diphtheria continue to appear among these children. Before these preparations could be completed, however, five more cases had developed, one on February 22d, one on February 24th, two on February 25th, and one on February 27th.

On February 27th and 28th, a second general inoculation was given, two hundred and forty-five children receiv-

ing the antitoxin, which was a product of the laboratory of the New York City Board of Health. In this instance the dose was increased by 25 units throughout, ranging from 125 to 225 units according to age. From this inoculation there was a period of complete immunity from diphtheria among these children extending over four and a half weeks, a case having developed on March 30th.

It could not be seen that antitoxin had any effect upon the Loeffler bacillus itself, since during the months of January and February the bacilli were found in so many of the children's throats it was necessary to extend the quarantine to nearly every building in the asylum, and, although no bacteriological examination of the throats was made at the time of inoculation, for want of time, yet one may be assured of the presence of the bacilli at that time, since they were found in nearly all at periods either immediately preceding or subsequent to the inoculations. That some of these bacilli were virulent, is proven clinically by the cases of true diphtheria occurring in adults and children (the latter being cases of reinfection) during the periods of immunity following the first and second inoculations; and experimentally by the New York City Board of Health in the guinea-pig.

The ill effects from the inoculations were limited to a few cases of pronounced urticaria after both general inoculations, and a few needle abscesses after the first, consequent upon haste. Constitutional symptoms with temperature, etc., were almost *nil.*

Nothing more can be said for antitoxin as an immunizing agent against diphtheria, except that the results obtained, as stated, cannot justly be attributed to coincidence.

RESULTS OF AUTOPSIES ON CASES DYING AFTER TREATMENT WITH DIPHTHERIA ANTITOXIN.

BY GEORGE P. BIGGS, M. D.

With so broad a field for discussion as has already been opened by the papers listened to this evening, it would be out of place for me to enter into an exhaustive consideration of the gross and microscopic lesions found after death from diphtheria, especially as the majority of those present are probably interested more in the clinical than in the pathological phases of this disease. I shall therefore confine myself to a brief review of the results of diphtheria autopsies made by myself at the Willard Parker Hospital since my appointment as assistant pathologist to the New York City Board of Health. Lack of time, necessitated by so early a report, has prevented as elaborate an analysis or as critical a study of these cases as could be desired.

Eighteen autopsies have been performed, two in the latter part of February, and sixteen in March, this number representing about 75 per cent. of the deaths from diphtheria occurring during this time. As thorough bacteriological examinations have been made in but a small proportion of cases, my report will be only of the pathological findings, particularly those having important bearing on the clinical course of the disease. In each case gross lesions have been recorded by dictation during the performance of the autopsy, and portions of the various organs have been subsequently studied in the fresh state by making frozen sections, and later in hardened sections from Müller's fluid and alcohol.

We may class the lesions found under four heads, viz.:

I. Local lesions in the nares, pharynx, tonsils, larynx, and trachea.

II. Lesions constantly present in varying degree in the internal organs.

III. Complicating lesions not invariably present.

IV. Lesions of antecedent disease.

I. *Local Lesions.*—In 6 of the 18 cases no membrane whatever was found. Five of these had been cases of laryngeal diphtheria, and slight ulceration of the larynx, chiefly of the vocal cords, still persisted in three of them.

Membrane, when present, was comparatively thin, in most cases 1 to 2 mm. in thickness, and as a rule loosely attached to the underlying tissues except in the larynx, where it was usually quite adherent over the vocal cords at least. In two instances, however, tonsillar membrane was found very adherent, but these were in patients who received antitoxin for the first time on the sixth and eighth days of the disease, and did not respond to treatment.

Clinically, 13 of the patients were distinctly croupy, and in one instance the disease was exclusively laryngeal in character. The autopsy records show 2 additional cases in which the larynx was involved, making a total of 15 out of 18 cases in which the larynx was invaded.

Of the 12 cases in which membrane was found at autopsy, in only 2 was the larynx free from involvement. One of these cases was three years of age, was admitted to the hospital on the seventh day of the disease, and died two days later. The autopsy showed an acute exudative nephritis and miliary tuberculosis. The second case was a child three and a half years of age, who was admitted on the fourth day in a very septic condition, with extensive gangrenous process in the throat, and who died within a few hours. Eight of the ten laryngeal cases had required intubation, but only one tracheotomy. In four the membrane was limited to the larynx, or larynx and upper

trachea, and in six it covered the pharynx and tonsils also. Deep necrosis of the tonsils was found in two cases, and both of these were clinically decidedly septic. Distinct enlargement of the cervical lymphatic glands usually existed, but was at times only slight.

II. *Lesions Constantly Present in Other Organs.*—The lesions constantly found in the internal organs were chiefly parenchymatous and fatty degenerations of the heart, liver, and kidneys. These changes have been more or less prominent features in all the cases, though the degree of degeneration has varied within wide limits. In a general way it may be stated that they were more advanced the later the cases came under treatment. Those treated early showed a mild degree of degeneration, while in three cases first receiving antitoxin on the seventh, seventh, and eighth days respectively, there was extreme fatty degeneration.

These facts point clearly to a marked influence exerted by antitoxin, when administered early, in checking and preventing these destructive changes.

III. *Complicating Lesions Not Invariably Found.*—These were most frequent in the lungs. Bronchitis of greater or less severity existed in all cases, twelve being characterized by abundant purulent secretion with intense inflammation of the bronchial and peri-bronchial structures, and six by muco-purulent secretion, with inflammation of the bronchial mucous membrane only. Distinct membrane was found in the bronchi in only one case.

Pneumonia in some form was found in all but two autopsies, the consolidation being lobar in one instance and broncho- or lobular in the remaining fifteen. The lobar consolidation developed several days after complete recovery from diphtheria, and was the sole cause of death. Excluding this case, the percentage of broncho- or lobular pneumonias was eighty-eight, a proportion much higher than that usually given. This fact would tend to show

that the improvement in mortality under the antitoxin treatment of diphtheria is due to its influence on cases not complicated by broncho-pneumonia.

Eleven of the broncho- or lobular pneumonias may be classified as extensive (involving one third to one half of each lower lobe, and usually smaller portions of the middle and upper lobes), two as moderate in extent, and two as slight.

Acute fibrinous pleuritis complicated the case of lobar pneumonia and one case of extensive broncho-pneumonia. Thrombi were found in the small pulmonary vessels but once. The spleen was seldom enlarged or softened, but usually congested.

In two cases the kidneys, in addition to advanced parenchymatous degeneration, presented the typical lesions of acute exudative nephritis. In each instance the patient did not receive antitoxin until the seventh day of the disease, and death occurred two days later.

IV.—*Lesions of Antecedent Disease.*—Lesions of antecedent disease which could be looked upon as important factors in explaining the fatal termination, have been chiefly of a tubercular character. General miliary tuberculosis existed in two of the cases; tuberculosis of the lungs, bronchial glands, and liver, in one case; and of the lungs and bronchial glands in two others. Syphilitic cirrhosis of the liver was very marked in one instance and slight in another.

The unfavorable conditions under which treatment was begun, in many of these cases, may be judged from the following facts:

1. *Age.*—The average age of the patients was two and a half years, the oldest being five and a quarter years, and the youngest eight months. Eight were two years old or younger, 6 were between two and three years old, 2 between three and four years, 1 five years, and 1 five and a quarter years.

2. Duration of the Disease Prior to Administration of Antitoxin.—This averaged 4.4 days; 2 receiving treatment on the second day, 4 on the third, 5 on the fourth, 2 on the fifth, 2 on the sixth, 2 on the seventh, and 1 on the eighth day.

3. Location of Membrane.—As already stated, 15 were laryngeal cases; 10 requiring intubation, and 1 tracheotomy.

Taking an average of all cases, death occurred seventeen days from the beginning of the disease. Excluding three, which terminated fatally within twenty-four hours after admission to the hospital, the average period is extended to nineteen days from the onset of the disease.

In order to ascertain the number of apparent failures of antitoxin in the treatment of these patients, we must first exclude from the series two cases in which death was not the result of diphtheria or of any of its direct complications. One of these died of typical lobar pneumonia, which developed several days after complete recovery from diphtheria, and the other of marasmus and prolapse of the rectum three weeks after disappearance of Klebs-Loeffler bacilli from the throat. No membrane was found in either case at autopsy.

Of the sixteen cases in which the immediate cause of death was diphtheria or some of its complications, the following should, for obvious reasons, be excluded from those in which the antitoxin treatment may be considered to have failed:

1. Two cases complicated by general miliary tuberculosis, which it is fair to say would have been hopeless under any method of treatment. These patients first received antitoxin on the fourth and seventh days respectively.

2. One in which no antitoxin was given, owing to the fact that a second and fatal attack of diphtheria developed so insidiously as not to be recognized, when the patient had almost recovered from a severe broncho-pneumonia.

This had complicated an attack of diphtheria occurring four weeks before.

3. Two cases, treated on the sixth day, with a serum of entirely inadequate strength.

4. Two admitted to the hospital on the fourth day of the disease, in a state of profound sepsis, and dying, one a few hours after admission, and the other within twenty-four hours.

Among the 9 cases still left for consideration, there are 3 in which no diphtheritic membrane was found at autopsy. Death was accounted for in 2 of these (both treated on the fourth day) by extensive broncho-pneumonia, and in the third by extensive fatty degeneration of the heart, acute exudative nephritis, and the septic condition of the patient when admitted on the seventh day of the disease.

There remain 6 cases, all with laryngeal involvement and requiring intubation, and all complicated by more or less broncho-pneumonia. In 3 of these, first treated on the third, fourth, and eighth days respectively, broncho-pneumonia was most extensive, and amply explained the fatal termination. In the three cases remaining the broncho-pneumonia found was not sufficiently extensive of itself to explain death. The following facts, derived from the clinical histories, have a bearing on the fatal result. One was a child, two and a half years of age, admitted after three days' sickness, with marked obstruction requiring immediate intubation, and a little later tracheotomy. Death occurred two days later. The second case, one year of age, was admitted five days after the onset of the disease, and died in two days. The third case was aged but eight months, was admitted after five days' sickness, and died within twenty-four hours.

Although the number of autopsies reported is comparatively small, the writer feels justified in making the following deductions:

1. That antitoxin has a decided influence in loosening diphtheritic membrane, even in cases resulting fatally.

2. That it has comparatively little influence on the cases already complicated by broncho-pneumonia.

3. That it has a marked influence, when administered early, in preventing and checking parenchymatous and fatty degenerations.

ON THE ANATOMICAL LESIONS FOUND IN HORSES DYING THROUGH THE COURSE OF PRODUCTION OF DIPHTHERIA ANTITOXIN.

BY IRA VAN GIESON, M. D.

The only constant lesion discovered at the autopsies was a very intense and quite general swelling of the lymph nodes. The lymph nodes of the brachial region, the inguinal region, as well as the lymph nodes at the root of the lung, those in the neck, at the hylus of the kidney, and elsewhere in the abdominal cavity, were very much swollen, red, and of diminished consistence. The lymph nodes near the site of the injection, were particularly marked as to these characteristics.

The heart, in three of the autopsies, was of a yellowish color, slightly softer than normal, and somewhat friable. In one of the cases there were two or three yellowish-white nodules about a centimetre and a half in diameter, situated at the lower termination of the anterior border of the right lung.

When the horse had died, recently, after an injection, the subcutaneous connective tissue about the region of the injection was œdematous and of a gelatinous color and consistence. In one or two cases white, hard nodules in the subcutaneous connective tissue or in the underlying muscular tissue, marked the sites of older points of injections.

Microscopical examination of the various organs and tissues gave the following results:—The brain, cerebellum, medulla, and spinal cord, as well as the peripheral nerves, were normal. In no case were there any lesions of

the central or peripheral nervous systems, except in one instance where there was a distention of many of the smaller veins in the cortex by masses of polyneuclear leucocytes. The heart muscle fibre in those cases where the organ appeared abnormal at the autopsy, showed fatty degeneration with osmic acid preparation.

The nodules in the lungs referred to in one of the cases, were composed of air cells densely packed with leucocytes and other small round cells. The various lymph nodes show an enormous increase of lymphoctes, and, in many places, the sections are densely crowded with polyneuclear leucocytes and eosinophilous cells. The submaxillary glands and the pancreas show no changes; the spleen in many of the cases, is very densely crowded with red blood cells, and shows less plainly the characters of the cells constituting the splenic pulp; the liver shows quite uniformly minute areas of necrosis, and such necrotic areas have intermingled with, and surrounding the dead liver cells, moderate numbers of eosinophilous leucocytes; the kidneys show the changes corresponding to acute parenchymatous nephritis, and in one case there are in addition, areas of small round cells interspersed between the tubules and surrounding the glomeruli.

The characteristic changes, then, in these horses, are the proliferation of cells in the lymph nodes, and the minute necrotic areas in the liver.

DISCUSSION.

DR. J. E. WINTERS: Some months ago I shared with my colleagues the hope that a specific against the toxins of the diphtheria bacillus had been discovered, but three months of daily observation in the Willard Parker Hospital has been a sad—an extremely sad—disappointment to me, and I regret to say that, as a result of that experience, I am here to-night to speak in opposition to the antitoxin treatment of diphtheria. My remarks will be based entirely on three months of daily clinical observation of the effects of the antitoxin in one hundred and fifty-four cases of diphtheria treated in the Willard Parker Hospital. In the paper of the evening reference has been made to the effects of the antitoxin on the pseudo-membrane of diphtheria. What I have to say with reference to this may be best illustrated by referring to four cases which were recently admitted into the Willard Parker Hospital in the course of one day.

CASE I had a few patches of pseudo-membrane in various portions of the throat, showing that the exudate had already exfoliated, only small portions of it remaining.

CASE II had pseudo-membrane on both tonsils, which was thick at the edges, and exfoliation had already begun, in some places being detached from the mucous membrane one eighth of an inch.

CASE III was in the early stages of the disease process. The pseudo-membrane was thin at the edges, involving the tonsils, and, slightly, the pillars of the fauces. At one point there was inflammation at the edge of the pseudo-membrane.

CASE IV, a septic case, with an abundance of membrane on the tonsils and pillars of fauces, and some on posterior wall of pharynx.

In Case I, where detachment of membrane was nearly complete before patient was brought to the hospital, the throat cleared rapidly. In Case II, the exfoliation, which had begun at the time of admission of the patient, continued, but some membrane remained on pillars of fauces, and on tonsils ten days after admission. Case III was free from membrane for first time on tenth day after admission. Case IV, the septic case, lived three days.

During this time, though thoroughly under the influence of antitoxin treatment, the antitoxin never made the slightest impression on the symptoms of toxæmia. These four cases may be taken as a sample of what I have seen repeatedly at the Willard Parker Hospital during the three months of antitoxin treatment of diphtheria. In not a single case has there been the least evidence that the formation of the pseudo-membrane was checked, that the exfoliation of the pseudo-membrane was hastened, or that the throat was free from membrane earlier than in cases which have not been treated by antitoxin. In not a single septic case has the antitoxin made the least impression on the symptoms. The toxæmia has not in one instance been relieved or lessened. There has been no indication in the character or frequency of the pulse, or in the general condition of the patient, that a specific for the toxæmia had been administered.

Reference was next made in the paper of the evening to the laryngeal cases of diphtheria, and I should like to ask the author of the paper and those who are supporters of the antitoxin treatment, why it is that antitoxin should be administered for the relief of the laryngeal stenosis due to a diphtheritic pseudo-membrane? While the diphtheritic process remains confined to the laryngeal mucous membrane, there is no toxæmia. There is, therefore, no indication for the exhibition of an antitoxin. It has been stated in the paper that the death-rate in the laryngeal cases under the antitoxin treatment has been lower than under the methods of treatment in vogue before the use of antitoxin, but in so doing the author compares results one year ago with the results during the past three months. The author has neglected to state to the Academy that at the Willard Parker Hospital, in the month of December last, we had eight *consecutive* recoveries after intubation in cases of laryngeal diphtheria without antitoxin. During the same month at the Foundling Asylum in this city, out of fourteen cases of intubation for laryngeal diphtheria, there were twelve recoveries without antitoxin. At the Willard Parker Hospital in the month of December, 1894, the recoveries from laryngeal diphtheria without antitoxin were about 75 per cent. This has never been equalled with the antitoxin treatment with or without intubation.

The paper of the evening deals largely with statistics.

I wonder if the members of the Academy have noticed that the author treats the matter of statistics largely by elimination and exclusion. In comparing the results of antitoxin treatment with the former methods of treatment, in the antitoxin cases, he excludes numerous deaths for one or another reason, while in speaking of the results before antitoxin treatment no deaths are eliminated. The facts are that during the first three months of 1894 the death-rate at the Willard Parker Hospital without antitoxin was 32 per cent. During the first three months of 1895, with antitoxin treatment, the death-rate was 28 per cent. But in order to make it clear to you that this mortality rate does not represent the exact state of things as it should, I will ask you to consider for a few moments the class of cases which have been brought into the Willard Parker Hospital for antitoxin treatment during the past three months. In many of these cases there has been no clinical evidence of diphtheria. They were cases of diphtheria from the bacteriological standpoint only. Examination of the nose and throat, examination of the constitutional condition of the patient, revealed no evidence of diphtheria. They had in the throat the Klebs-Loeffler bacillus, but the presence in the throat or in the nose of this bacillus, in the absence of any lesion, is not evidence of the existence of diphtheria. *I should like to know why antitoxin is used in cases where the presence of the Klebs-Loeffler bacillus is the only evidence of diphtheria, when it is a well-established fact that the antitoxin treatment has no influence on the bacillus—it persists as long in cases treated with antitoxin as it does in cases treated without it.*

During the first three months of 1894 no case of diphtheria was seen in the Willard Parker Hospital that did not have abundant clinical, as well as bacteriological, evidence of diphtheria. These cases of so-called "diphtheria" without any of the clinical manifestations of the disease, should be eliminated from the statistical report, and there have been a score or more of such cases during the past three months, and were these cases eliminated from the list, the death-rate under antitoxin treatment during January, February, and March, would be much higher than it was during the corresponding months of 1894; and this, too, at a time when diphtheria is far milder than it was one year ago. Some of the cases brought to the hos-

pital in February, and many of those admitted in March, are still very ill and will certainly die. Were these included, the mortality during the antitoxin treatment would be startlingly high.

The author of the paper, like all others who have written about the antitoxin treatment of diphtheria, has insisted upon early treatment as a *sine qua non* for the best results, but the record from which Dr. Biggs has prepared his paper will show that the first two cases on the list which had recovered, had their first injection of antitoxin on the eighth and sixth days respectively.

In the next seven cases on the list the first injection was given on the fourth day in two cases, on the fifth day in two cases, on the third day in two cases, on the sixth day in one case, and all died. Case XXIV on the list had first injection on the ninth day and recovered. One case, injection on second day, died. One case, injection on tenth day, got well, and one injected on the third day died; and so on throughout the list. It is not the date of first injection which determines the result, but it is the individual character of the case and the nature of the disease.

At the Willard Parker Hospital in any case which, at the time of admission, has given evidence that the disease was progressive in its course, that course has not been stayed or changed in any particular by the antitoxin treatment.

It has also been stated that the serum used in the early part of the antitoxin treatment at the Willard Parker Hospital was so weak that little could be expected from it. A careful study of the record will show that the cases which were treated with the weaker serum did better than those which were treated with the stronger serum. Again, a study of the records will show that those who had the smaller dose of serum did better than those who had a larger dose. In fact, a careful study of the individual cases of diphtheria treated with antitoxin in the Willard Parker Hospital will show that there has been no relation between the antitoxin treatment and the recoveries. As to the early treatment of diphtheria with antitoxin, it may be definitely and confidently stated that diphtheria at the present time, almost invariably recovers under any method of treatment if brought under proper hygienic surroundings and surveillance on the first day of the disease. Diphtheria is scarcely ever fatal if the patient is put to bed and

properly cared for on the first day of the illness. It is the *insidiousness* of diphtheria which constitutes its greatest danger.

Now as to some of the untoward or unfavorable effects of the antitoxin treatment. The rashes have been spoken of in the paper, and some of the minor features attributable to antitoxin, but the more serious effects of the antitoxin treatment have not been alluded to.

I will ask you to go with me for the time being to the Willard Parker Hospital and see some of the effects of antitoxin which I have witnessed there. Babies have been brought to that hospital with slight clinical evidence of diphtheria—babies, with the exception of a slight exudate in the throat, and perhaps a slight croupy cough, that were apparently healthy and with rosy color. They remained in good condition for perhaps ten days or two weeks after receiving antitoxin, when one day as you enter the ward you hear a moan, and approaching the crib you find the baby lying on its side with its head retracted, its arms and legs flexed, and moaning piteously. I can best make it clear to you what this baby's condition is by relating to you a case about which I was consulted. A physician came to my office to advise with me regarding his wife. Two weeks previously this lady had had a slight sore throat, and a medical friend had immediately injected her with antitoxin. The following day it was decided that she had no diphtheria, but a slight tonsillitis, and the throat was all but well. Fifteen days after the injection of antitoxin her temperature was 105° F., pulse 160, and she was suffering severe pain in all her joints. Sleep and rest were absolutely impossible except as obtained by large doses of morphine administered hypodermically. This was a case of antitoxin infection. In her case the antitoxin was eliminated and the patient recovered. This is the condition in which I have seen many of the little babies in the Willard Parker Hospital. The temperature chart shows a record of 105° to 106.8° F., and a note of the night nurse tells that the patient has scarcely slept at all. This antitoxin infection pursues one of two courses, either the symptoms may gradually subside, leaving the baby emaciated and extremely anæmic, or there may be a fluctuating temperature, ranging from 99° to 106.8° F., and existing for days together. What is this condition? It is

due to the action of the antitoxin on the blood. *Horse serum dissolves human blood corpuscles, and thereby produces new elements of decomposition. The condition of the babies just spoken of was one of antitoxin septicæmia, and brought about by the influence of the antitoxin on the blood.* A further evidence of septicæmia was in occasional suppurations, as seen in one case in the neck and ear, in another in small points of suppuration in various portions of the body. In these cases we have a temperature range such as has not been seen in the hospitals of this city since the introduction of aseptic surgery. This is a temperature range such as we used to have in the surgical and maternity wards of Bellevue Hospital twenty years ago.

It has been said here to-night that many of these cases treated with antitoxin have not died from diphtheria; that an autopsy has proven that the babies had recovered from diphtheria, and that the lesions proved that they had died of pneumonia after recovery from diphtheria. *This pneumonia in the Willard Parker Hospital during the past three months has come on late in the course of the disease, and it has been such as seen in Bellevue Hospital when they were having a great deal of septicæmia there. It is the pneumonia of septicæmia, and this septicæmia was induced by antitoxin.*

A second form of antitoxin infection was seen in the case of a little girl, aged thirteen months, admitted March 22d. Given the first injection of antitoxin immediately; temperature before injection 101.6° F. Temperature six hours after injection, 104° F. Second injection, March 23d; afternoon of same day, kidneys ceased to act. Hot pack and other methods of treatment resorted to. Baby died in spite of all treatment, on the afternoon of March 26th.

Dr. Lennox Browne has reported a total of eight cases of diphtheria treated with antitoxin; death resulted in six cases, all due to inflammation of the kidneys. Benda found nephritis in thirty-three cases out of thirty-nine who had died under the antitoxin treatment.

An instance of antitoxin infection through its action on the nervous centres occurred in a baby, aged two years and four months, admitted March 19th. Temperature, 101.2° F. on admission. First injection of antitoxin at 6.30 P. M. Baby had a restless night. At 5 A. M., on

the 20th, temperature was 105° F.; at 5.45 A. M., baby was in convulsions, and died at seven o'clock in convulsions, death resulting thirteen hours and fifteen minutes after injection of antitoxin.

The third case of immediate bad result with antitoxin was in a baby, aged eleven months, admitted March 29th. Temperature, 101.8° F. on admission. First injection, day of admission; second injection, following day. About twenty-four hours after admission, baby's temperature was 105.8° F.; convulsive movements of the eyes were noticed. Careful examination revealed no evidence of organic disease. Baby was in semi-stupor, respirations twenty-eight to the minute. Pulse rapid, irregular, and intermittent. March 31st, temperature, 106.8° F.; convulsions general. Baby died 2.30 A. M., April 1st., in convulsions. Temperature, 108 F. an hour before death.

Autopsies asked for but not granted in any of these cases. Physical examination revealed no evidence of organic disease in either of these last two cases; they were unquestionably cases of antitoxin poisoning.

This brings us to one of the most important considerations in connection with the subject of antitoxin treatment of diphtheria, namely, individual susceptibility to the action of remedies.

There is in this room to-night a physician who, about a year ago, came very near dying from the influence of a single dose of 1-150 grain of atropine. He is organically healthy, and was well at the time with the exception of a slight coryza. Any one of us in this room might have taken the same dose or double the amount without suffering any ill effects. This is one instance of individual susceptibility. As we all know, one person is specially susceptible to the influence of morphine, another to the influence of atropine, and so on throughout the entire list of remedies. So some individuals are specially susceptible to the influence of antitoxin. When this susceptibility exists, or to what extent, can never be known beforehand. It was the existence of this special susceptibility which caused the antitoxin to produce its fatal effects in the cases here enumerated. It was the existence of this special susceptibility or idiosyncrasy in the case of Miss Valentine which caused her unfortunate death from a single injection of antitoxin. That the antitoxin which was used in her case

was all right in every way has been officially certified to by the Board of Health of this city and of Brooklyn. The fault was not in the antitoxin used, but in the special susceptibility of the patient. This same antitoxin might have been used on another person without producing any ill-effect. We know nothing of this individual susceptibility beforehand, and any physician who uses antitoxin takes the risk of bringing about the results which I have described to you as having occurred in the Willard Parker Hospital, and the result which occurred in the case of Miss Valentine.

A case parallel to these is reported in the *British Medical Journal* of March 2d, p. 505. A well child, three years of age, was given an immunizing dose of 2 c.c. of No. 1 Behring serum. The next morning the child was depressed. On the following day she had fever and violent pain in the lumbar region, and the urine contained a considerable quantity of albumin. The next day the temperature was 103° F.; petechiæ appeared on the legs, and the day after the child died.

In summarizing, I oppose the antitoxin treatment of diphtheria, first, because in an experience of one hundred and fifty-four cases during the months of January, February, and March, 1895, in the Willard Parker Hospital, I have failed to see the slightest evidence that it neutralized the toxæmia in a single case. I have never found that it exerted the slightest influence for good in a single clinical manifestation of the disease—on membrane, pulse, temperature, gland swelling, larnygeal symptoms, etc. Every one of these cases has been examined daily; they have been examined with care and with great solicitude, and with a desire to discover some evidence of the virtues ascribed to antitoxin by others.

Secondly, *I oppose it on account of its immediate danger to life, through its influence on the kidneys and on the nervous system, and remotely, through its influence on the blood.*

Dr. G. L. Peabody.—The hour is so late that I will will not take up much of your time. I think enough has been said already to convince us that we have a very complicated problem to deal with. In judging the results obtained by means of antitoxin, we must know all the individual facts in each particular case in which it is

employed. The bacteriological diagnosis of diphtheria, as Dr. Winters has said, is very often both practically and clinically, exceedingly unsatisfactory. In one instance coming under my observation, almost an entire family was pronounced bacteriologically diphtheritic, and yet the disease did not develop in a single one of them. I was called to attend a governess in this city who had charge of four small children. The governess complained of a sore throat, and on inspection it was found that both the tonsils and soft palate were covered with false membrane. I took cultures from her throat, as well as from those of the four children, which showed nothing abnormal, and the bacteriologist reported that the cultures from the governess showed no Klebs-Loeffler bacilli, while those from three of the children's throats did.

While such an occurrence as this does not in any way invalidate the work of the bacteriologist, it shows that we must look for something besides the bacilli. The observations that have been made seem to show that while the bacilli may be present without having diphtheria, there can be no diphtheria without the bacilli. I saw a case not long ago in which the Klebs-Loeffler bacilli were found in the throat three or four months after the membrane had disappeared. I am not willing to keep such a person exposed to the wards of a contagious disease hospital, and still I am unwilling to give him a clean bill of health. It is probable that with diphtheria, as with tuberculosis and other diseases, the mere presence of the germ is not sufficient to cause the disease; it requires in addition that vague condition on the part of the patient which we call receptivity.

Although my personal experience with antitoxin in the treatment of diphtheria has not been large, it has in the main been very satisfactory. I have seen convincing proof to my mind that, even under the influence of a single injection, the membrane melts away more quickly than I have ever seen it disappear under any other form of treatment, either local or constitutional. As regards its untoward effects, I have seen one case in a neighboring town in which distinct septic symptoms developed, such as Dr. Winters has described. In that instance the patient ultimately recovered.

It is possible that Dr. Winters's poor opinion of the antitoxin treatment may be explained on the supposition that

the cases in which he saw it employed were not cases of pure diphtheria. Antitoxin will not affect the cases in which there is a streptococcus or mixed infection.

DR. L. EMMETT HOLT.—So far as I am concerned, I am satisfied that the antitoxin treatment of diphtheria is of value. In no instance have I seen any bad results follow its use. I have employed it in about twenty cases. These patients were nearly all young children and were treated in institutions in which the mortality of the disease last year was about seventy per cent. This year, every case that was injected with antitoxin on the first or second day has recovered, with one exception, and that was a child suffering from marasmus, which died five or six hours after the injection was made.

I wish to enter a protest against the slur cast by Dr. Winters on the value of the bacteriological diagnosis. Without it we can never be quite sure. But the bacteriological examination alone is not sufficient, since it is certain that bacilli are very frequently found without any evidence of disease. A systematic examination of children admitted to the Babies' Hospital during the past few months has shown the presence of the Loeffler bacilli in about one third of the total number. In order to know that a patient has diphtheria, we must have both clinical and bacteriological evidence.

In reply to the statement made by Dr. Winters that lately in almost every case referred to the Board of Health for a bacteriological diagnosis, the report was returned "true diphtheria," I would state that during the past month I have referred six or eight cases, every one of which was returned as non-diphtheritic.

In conclusion I wish to state that if one of my children was taken with diphtheria to-night, I should employ the antitoxin without hesitation.

DR. W. L. SOMERSET.—As an interne of the Willard Parker Hospital during the past sixteen months, I have had an opportunity to carefully watch all the cases that were brought there for treatment. I will state at the outset that this hospital is not conducted for the purpose of making statistics for antitoxin, but for the purpose of taking care of the patients sent there for treatment. Dr. Winters referred to certain patients treated there with what he

describes as very mild attacks of diphtheria—children with rosy cheeks and a croupy cough. I cannot recognize any case with a croupy cough as a mild case of diphtheria. In times gone by we had cases which showed an irregular range of temperature, we had the rashes, and we had other symptoms which were then ascribed to slight digestive disturbances but which are now charged to the antitoxin.

I beg leave to present the following figures, which are of some importance as bearing on this subject: Of twenty-four consecutive fatal cases of croup treated in 1894 (without antitoxin) thirteen died within twenty-four hours and eight on the second day; that is, 87½ per cent. died within forty-eight hours. Of twenty consecutive fatal cases of croup treated in 1895 (with antitoxin) two cases died during the first twenty-four hours and three during the second twenty-four hours; that is, only 25 per cent. died within the first forty-eight hours.

In autopsies made on fatal croup cases treated without antitoxin, I have noticed as a rule that adherent membrane existed below the larynx, whereas in recent autopsies on croup cases treated with antitoxin there was an absence of membrane below the larynx.

DR. WINTERS.—In reply to Dr. Peabody I would state that my remarks did not apply to cases of mixed infection. In reply to Dr. Holt I wish to say that I did not cast any slur upon the bacteriological diagnosis of diphtheria; what I said was, that a bacteriological diagnosis alone should not be sufficient grounds for sending a patient to a hospital for contagious diseases.

DR. BIGGS (closing the discussion).—Dr. Winters, in his very eloquent and dramatic exposition of the baneful effects of antitoxin, unfortunately omitted to explain why the mortality of diphtheria during the first three months of this year, eliminating nothing, is twenty per cent. lower than it was during the corresponding period last year. Any one who would deliberately make the statement that there is but little diphtheria of a severe type in New York city at the present time cannot be considered as an unbiased or unprejudiced observer. In this city last year there were 2,800 deaths due to diphtheria and croup.

I want to say that many of the statements and inferences

made by Dr. Winters are distinctly untrue. He said that of all the septic cases treated with antitoxin, not one has improved. If any one statement in connection with this subject has been emphasized from the beginning, it is that antitoxin is of absolutely no value excepting to neutralize the toxin of diphtheria and so to reduce inflammatory swelling. There is no reason to look for the relief of septic conditions from the action of antitoxin. In the statistics given there were no cases eliminated excepting those that died within twelve hours after treatment was commenced; these were justly eliminated, because antitoxin does not produce its effects within that time. In two other cases that were eliminated the treatment was discontinued.

Dr. Winters stated that recent diphtheria epidemics were mild in character. The reports made to the Board of Health show that the disease this year, until the antitoxin was used, was more fatal than last year. Not only in this city has antitoxin reduced the mortality rate, but in Paris and Berlin, and in other large cities where it has been employed. The statistics given, so far as I know, are made up only of cases which were reported to the Health Board by practising physicians as diphtheria, and in which the diagnosis of diphtheria was confirmed by the bacteriological examination.

I would like to know why horse serum, injected hypodermically into a human being, should dissolve the red blood corpuscles, as Dr. Winters claims it does. From the very beginning, Dr. Winters has shown a profound distrust of the new treatment of diphtheria. In conclusion, I have only to say that so far as I know there is to-day not a single distinguished clinician, pathologist, or bacteriologist in Europe who has not accepted this new treatment as one of the most important advances in therapeutics of modern times.

Stated Meeting, April 18th, 1895.

FURTHER IMPROVEMENTS IN MICROSCOPICAL TECHNIQUE.

By HENRY G. PIFFARD, A. M., M. D.

In the New York *Medical Journal* for July 16th, 1892, I published the description of an electrical illuminator for use with the microscope. The lamp was an incandescent, with cylindric bulb about three inches in length, and furnished with a short, and comparatively thick, carbon filament, supported in position by copper wires.

To bring the lamp to proper incandescence a current of three ampères, with pressure of fifteen volts, is requisite; and under these conditions it emits a light of fifteen candle-power. If the lamp were directly connected with the Edison service of one hundred and twenty volts it would be instantly destroyed by the excessive pressure, just as a water-pipe, intended to support a given pressure, would burst if subjected to a much higher one. The means first adopted by me to obviate this inconvenience was the insertion of a proper resistance-lamp in series with the illuminator. The lamps furnished at the beginning were quite satisfactory, but later I found great difficulty in obtaining them of uniform resistance and voltage. This, on the whole, was rather fortunate, as it compelled me to look elsewhere for some other means to effect the same result. This I found in a Carpenter rheostat, which not only answers the purpose in the most satisfactory manner, but possesses certain special advantages in addition. The object of the lamp here described was to obtain critical illumination under the best conditions and with the least

inconvenience. (The lamps and rheostat can be obtained from The General Electric Company, New York.)

Microscopical illumination is said to be critical when the minified image of the radiant coincides in plane with the object under examination; in simpler words, when the lamp-flame is focussed on the object. The means most commonly employed to obtain critical illumination by expert microscopists in the use of the narrow edge of a small (half inch) kerosene flame, such as is furnished by the Beck (English) and Acme (American) and other microscope lamps. The rays from this flame should be parallelized by a suitable convex lens, and then focussed by the concave mirror (without condenser) or with plain mirror and achromatic condenser.[1] The advantages of critical illumination reside in the fact that in many cases fine details of structure are displayed to better advantage than when the light is not critical.

Referring again to the rheostat, the one here used contains twelve steps or grades of resistance, the maximum being forty ohms. With the entire resistance in circuit the lamp burns with a fairly bright light of slight yellowish tint. As the arm is moved forward step by step the light increases in brilliance until the sixth step is reached, at which point the intensity becomes dazzling and beyond which it is not prudent to go, that is if the life of the lamp is to be preserved for any length of time.

The quantity of light thrown on any subject under examination varies with the quantity emitted from the original source, controlled in turn by the diameter of the diaphragm, the intensity of the light remaining the same; and it is well known that certain objects become visible only when the quantity of light is diminished by a small diaphragm. When the intensity varies, however, the case is quite different, for we find that in a large proportion

[1]Critical illumination cannot be obtained when using the chromatic or so-called Abbe condenser.

of objects fine detail is displayed to better advantage by increase rather than diminution of the intensity. This opens a field of investigation in connection with microscopical illumination that has not been, so far as I am aware, hitherto explored. My own experience with it, limited to a few months only, is insufficient to enable me to speak in detail of all the benefits that may be derived from having the variation of intensity under such perfect control. I am satisfied, however, if the working microscopist once has this means of control at his command he will never afterward willingly give it up. My personal experience, moreover, is in full accord with theory. With light of lower intensity there is a larger relative proportion of rays of long wave-length, while with the higher intensity the light contains fewer rays of long wave-length and a relatively larger number of those of short wave-length. The symbol λ is usually employed to indicate the wave-length, and we have the equation $Rp=\frac{2\,N.A.}{\lambda}$ In other words Rp (resolving power) is greater as λ (wave-length) becomes smaller. For definition I prefer the longer wave-lengths; but for resolution the shorter ones are more effective. I therefore offer the arrangement here described as a distinct improvement over our present methods of microscopical illumination so far as the use of the artificial light is concerned.

In the *American Journal of the Medical Sciences* for July, 1893, I called attention to the advantages that would accrue both in visual and photo-micrographic work from having the objective corrected specially for the D line of the spectrum rather than for the shorter wave-lengths (F to G lines). Such lenses, performing in the most satisfactory manner, have been made for me and for others by Spencer, Gundlach, Bausch & Lomb, and Leitz, and can be obtained from them on special order.

In the *Medical Record* of March 24th, 1894, I described "A simple means of increasing the aperture and improv-

ing the performance of some immersion lenses." The lenses referred to were some English water-immersions, and certain oil-immersions of American make. The means adopted consisted simply in substituting the monobromide of naphthalin for the water or oil which the makers had originally intended to be used with their respective lenses. This substitution necessarily introduced a high degree of negative spherical aberration, which in turn was corrected by closing the systems and shortening the tube-length. By this means the numerical aperture was increased in the water-immersions in the ratio of 1.33 to 1.65, and in the oil-immersions in the ratio of 1.52 to 1.65. I further proposed that the opticians construct objectives primarily intended for use with the monobromide, but which could, if necessary, be used with oil. Dr. Dallinger, in the *Journal of the Royal Microscopical Society*, has criticised this idea unfavorably and irrelevantly, but I believe that if he makes the necessary practical tests he will change his views. I also communicated the idea here stated to a practical German optician of world-wide fame. The reply from Germany was definite and clear, and to the effect that the thing could not be done; that a lens could not be constructed that would work with mono-bromide, unless it was used in connection with objects mounted in media of high index on flint-glass slides and with flint-glass covers.[1]

A number of important improvements in connection with microscopy have been accomplished by American investigators, but almost without exception they have been re-

[1] "Der Vorschlag des Herren Professor Dr. Piffard wegen eines monobrom-Naphthalin Objectives ist zur Ausführung zu umständlich und solche waren auch in Gebrauch unpractisch. Diejenigen welche von Zeiss angefertigt werden leiden ebenfalls an diesem Fehler, so mussen z B. Condenser, Deckglass, und vordeste Linse aus schwersten Flint-Glass sein, dito Objecträger, und Einbettung in eine stark breckende Flüssigkeit (Realgar), was alles dem Optiker die Herstellung erschwert; und ein System in der angegebenen Weise hergestellt wurde die Leistungen der heutigen Oel Immersionen nicht ueberbieten."

ceived with ridicule and incredulity abroad. When many years ago the late Charles A. Spencer stated that he was able to construct well corrected objectives with an angular aperture exceeding 170°, the foreign criticisms were to the effect that he was either mendacious, or else that he did not know how to measure angular apertures. All the same, he sent a lens to England, the angle was measured, and his statements were confirmed.

When, a few years later the late Robert Tolles declared he had constructed a lens which practically possessed a higher aperture than that of the highest possible theoretical angle (180°) his statements were met with substantially the same reception. To-day we have in common use as the outcome of Tolles's ideas lenses with a numerical aperture from thirty to fifty per cent. higher than is possible with any dry lens possessing an angle of approximately 180°.

When also the late Colonel J. J. Woodward, M. D., asserted that he had resolved certain of Nobert's test-lines that had baffled European ingenuity, his reception was equally inhospitable. His photo-micrographs, however, settled the question.

Cognizant of the experience of other American investigators, I was quite prepared to encounter hostile criticism, but must confess I had some slight curiosity to learn the exact form it would take. I then conferred with an American optician, Mr. Herbert R. Spencer, of Buffalo, who constructed for me a special mono-bromide of naphthalin immersion lens to my entire satisfaction, and I will later show you (on the screen) a photo-micrograph taken with it of the well-known test object, the Pleurosigma angulatum. The object was an ordinary commercial Möller slide, attached to a crown-glass cover and mounted on a crown-glass slide, and in a medium of low index, namely air (n=1). This absolutely disproved the validity of the German criticism.

In my article in the *Medical Record (loc. cit.)* referring to substage condensers I wrote as follows:

The only existing strictly high-grade condensers that I know of are the achromatic and apochromatic condensers of N. A. 1.40 made by Powell & Lealand, of London. These will cost, laid down in New York, duty paid, about one hundred and twenty dollars each.[1] This price will prove prohibitory to many, and it is to be regretted that American opticians have not as yet found a way to produce a condenser of equal merit at a price that would bring it into more general use.

Since the above was written Spencer has accepted the challenge and supplied me with an achromatic condenser with N. A. 1.40, and at about half the cost of the English one. In its general performance and convenience of manipulation, it decidedly surpasses the Powell & Lealand achromatic N. A. 1.40 which I had at command for several months. Messrs. Watson & Sons, of London, recently informed me that they had just perfected an achromatic condenser with N. A. 1.25, but did not advise me as to the price.

In this connection I may be asked, What are the advantages that accrue from such large apertures in objectives and condensers? As regards objectives a two-fold answer may be given. First: Resolution varies in direct ratio with the numerical aperture, the greater the aperture the greater the resolving power or ability to distinguish and separate small particles from each other. This is a demonstrable and demonstrated fact and cannot be gainsaid. Second: The greater the aperture the more truthful the image; *i. e.*, the closer the agreement between the virtual image and the geometrical form and dimensions of the object in gross and in detail. This is not a demonstrable fact, but is a deduction from Abbe's theories of microscopical vision, and must be accepted *pari passu* with them.

[1] They are a little cheaper now.

To explain Abbe's theories in full with his demonstrations would require more space than can here be given. I shall offer, therefore, only a condensation from Davis.[1] This writer says:

Professor Abbe tells us "the very first step of every understanding of the microscope is, to abandon the gratuitous assumption of our ancestors, that microscopical vision is an imitation of macroscopical, and to become familiar with the idea that it is a thing *sui generis* in regard to which nothing can be legitimately inferred from the optical phenomena connected with bodies of large size."

From the researches of Professor Helmholtz and Abbe, it has been clearly demonstrated that images of bodies of large size, when formed by lenses free from chromatic and spherical aberration, are in themselves geometrically correct pictures, truly resembling the objects themselves, were they enlarged to the same size and illuminated in a similar manner. Pictures formed in this way are termed dioptric images. When, however, the object is made up of particles less than 1-2,500 of an inch separated from each other, it is no longer possible to depict such structure by ordinary dioptrical means. The resolution of structures made up of particles having smaller intervening spaces than this is effected entirely by what is called "diffraction," the broad outline being, however, at the same time depicted by dioptrical rays. The light in its passage through the object is modified, or scattered, and produces what are called diffraction spectra, which may be easily seen in the microscope when proper means are taken for their exhibition. If the ultimate particles of the structure are comparatively large, the diffraction spectra will most probably all lie within the field of the objective, while if they are small they may produce such a wide dispersion of the interference spectra that only a part of them may range within the field, the remainder being lost, owing to the aperture of the objective not being able to collect them. If only a portion of the diffraction spectra be collected by the eye-piece, it can scarcely be expected that the true structure is exhibited, and in fact this is the case, which can be easily exhibited by numerous experiments.

[1] Practical Microscopy, by George E. Davis, Am. Edition, 1889.

Professor Abbe further says that very minute structural details as a rule "cannot be interpreted as morphological, but only as physical characters; not as images of material forms, but as signs of material differences of the particles composing the object; so that nothing more can safely be inferred from the image as presented to the eye than the presence in the object of such structural peculiarities as will produce the particular diffraction phenomena on which the image depends."[1]

The value of high aperture in dry as well as in immersion lenses, is, I believe, underestimated by many laboratory workers who have followed the teachings and practice of continental laboratories rather than the dicta of science in this respect. The students of diatomology first learned to appreciate these lenses in their own special work, but the histologists and pathologists, or rather the majority of them, fail to convince themselves of their utility. They labored under a mistaken idea that penetration and depth of focus was a *sine qua non;* and as the penetration varies inversely with the numerical aperture, the lower rather than the higher ones were chosen. I allude more particularly to lenses of low and medium power, say from one inch to one-sixth inch equivalent focus. There were, however, two other reasons that kept the higher apertures out of the laboratory, namely, their greater cost, and the fact that medium powers required careful adjustment for thickness of cover-glass in order to obtain the best results that the lens is capable of giving. A 10 or 12 mm. lens with an angular aperture of 100° (N. A. .76) is an unknown construction on the continent; but such lenses may be readily obtained from American and English opticians.

Professor J. Edwards Smith[2] was, perhaps, one of the first to insist on the advantages of high apertures in his-

[1] Those desiring further information on this subject may consult Czapski: Theorie der optischen Instruments nach Abbe, Breslau. 1893.

[2] "How to See with the Microscope." Chicago, 1880.

tological work, and he is ably seconded in more recent times by Dallinger.[1] I desire to add my own experience and testimony in the same direction.

As regards the desirability of high-apertured condensers, it may be stated that they are necessary in connection with high-apertured objectives; that is to say, if the full working capacity of the objective is to be utilized, and an achromatic condenser bears a similar relation to a non-achromatic "Abbe" that an achromatic objective does to the old style French non-achromatic triplet, such as microscopists of fifty or sixty years ago were in the habit of working with. It should be remembered, however, that the aperture of the condenser should not be disproportionately large as compared with that of the objectives. If you attempt to pour a quart of beer into a pint measure, you will surely disorder the table or the floor, and if you use a condenser of N. A. 1.40 with an objective of N. A. .70 you will disorder the image in like manner. The advantages of an achromatic condenser compared with an Abbe will be appreciated only by those who have used both. The confusion of light rays that come through an Abbe condenser (especially when using artificial light) are to me at least as disagreeable and fatiguing as a Babel of sounds would be to a sensitive ear.

In the *Medical Record (loc. cit.)* I wrote as follows:

> The increased visibility given to certain structures by the use of mounting media of high index, is a subject that merits more attention than it has hitherto received, as it constitutes one of the factors that contributes to the best final result.

Diatom-workers have for years sought to improve their technique in this direction. Many diatoms are mounted "dry," that is, with air (n.=1.) as the medium; glycerine (n.=1.46) is never employed; Canada balsam

[1] Carpenter: "The Microscope and Its Revelations," edited by Dallinger. London, 1891.

(n.=1.54) is largely used; styrax (n.=1.58) from the *Liquidambar orientalis* Miller, and liquidambar from the *Liquidambar styraciflua* L. were found to be improvements; mono-bromide of naphthalin (n=1.65) gave still better immediate results, but the mounts are difficult to keep; iodide of methyl almost impracticable in use; phosphorus (n.=2.22) is dangerous; stannous chloride dissolved in glycerin jelly[1] and realgar, both introduced by Professor Hamilton L. Smith, possessed very high indices, the latter approximating 2.40. The higher the index, the more clearly the details of the diatom structure are shown, but as there are serious difficulties connected with the use of all the media of very high index, many diatom-mounters have adopted the mono-brom-balsam as the medium best adapted to general routine use. This preparation consists of a solution of hard Canada balsam in mono-bromide of naphthalin, and the index will approximate 1.60, being a little higher or lower according to the proportion of the respective ingredients in the mixture.

In biological, histological, pathological, and bacteriological work, many of the media above named are impracticable, and the almost universal mountant is Canada balsam, thinned with turpentine, chloroform, benzol, or xylol.[2] A few experimenters, looking for something better than balsam, have tried "mono-brom-balsam," but report it unsatisfactory.

Being fully aware of the advantages of some of the higher media in connection with diatoms, I have, during the past three years, given such time as I could spare to the search for better media for histological work. Styrax and liquidambar were tried and found better than balsam, but not quite good enough. Mono-brom-balsam was then employed, and exhibited manifold advantages over the

[1] Index will vary according to method of preparation.

[2] In one laboratory in this city glycerin is used to the exclusion of balsam, a procedure that owing to the low refractive of the medium, is unphilosophical in the extreme.

others, exhibiting histological details and bacteria very much better than balsam, and not open to the objections (lack of permanency, loss of refractive index, etc.) that have been urged against it. For temporary mounts demanding immediate examination its advantages over the ordinary balsam are very evident. In connection with permanent mounts there is one inconvenience. The mixture dries very slowly, and even weeks after mounting the slightest force will displace the cover-glass from its position on the slide. The best procedure is to carefully centre the cover on the slide, and then put it away for a week or two, after which adjust the slide on the turn-table and spin a ring of cement (shellac is best) around the edges of the cover.

Instead of using balsam dissolved in mono-bromide, a more desirable mixture, and one of higher index, may be prepared by substituting purified liquidambar for the balsam.[1]

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[1] Purified liquidambar is not an article of commerce, but may be prepared in the following manner:

Procure a pound of the crude gum (an exudation from the *Liquidambar styraciflua* L.) and put it in a water-bath to liquefy. Arrange also a Plantamour's jacketed funnel, with glass funnel inside, and two or three thicknesses of cheese-cloth. When the gum is sufficiently liquefied, and the Plantamour funnel hot, proceed to filter. The filtrate should be received in a quart bottle. When cold add one pound of xylol. This should be mixed as thoroughly as possible with the filtered gum, and well shaken with it several times a day for at least two weeks. At the end of this time filter the mixture two or three times through absorbent cotton or Swedish paper. The product will be a thin but perfectly clear and transparent fluid. This must now be concentrated by gentle heat, and the process may be considered complete when the warm liquid has about the consistency of thick molasses. When cold it forms a semi-solid mass extremely sticky and tenacious. Two parts of this should be mixed with three parts of mono-bromide of naphthalin, and a little gentle heat applied. The result will be a clear amber-colored fluid. Its refractive index should then be tested and brought to 1.625 by the further addition of whichever of the ingredients may be necessary.

Care should be taken to use the proper gum, as above indicated, which is the American "sweet gum;" and not the gum from either the *Styrax officinale* L., nor the *Liquidambar orientalis* Miller.

As to the permanency of this preparation I can only say that I have mounts made with it two years ago which as yet show no signs of deterioration.

The advantages of the latter preparation over the mono-brom-balsam are twofold: 1. When the liquidambar is mixed with mono-bromide of napthalin in the same proportions as the balsam, the refractive index is higher (n.=1.625) and we have increased visibility of minute bodies of lower index (tissues, bacteria, etc.).[1] 2. The mixture is more sticky and adhesive than the mono-brom-balsam, and the mount can be finished for permanent preservation in a shorter time.

In offering, therefore, the "naphthalin-ambar," I present it as the mounting media of highest index thus far produced, that can be put to practical use in bacteriological and histological work, and consequently the one best calculated to exhibit the intimate structure of this class of objects.

A very considerable portion of the laboratory work of the present day consists in the examination of sputum, and other bacterial mounts for diagnostic purposes only, and without any intention of preserving the slides. The medium most commonly employed in this connection is the ordinary immersion oil, with a refractive index of a little

[1] The relation of the refractive index of the mounting medium to the indices of microscopic objects has not received the general attention it deserves. On this point Van Heurck, referring to diatoms, writes as follows: "When any substance is immersed in a medium it becomes more or less visible, according to the difference of their respective refractive indices. Taking the index of diatom silica at 1.43 and water at 1.33, the number ten will express the visibility of diatoms in water. Canada balsam has an index of 1.54, which would give eleven degrees of visibility. Pure styrax has an index of 1.60, and consequently the visibility would be 17, while mono-bromide of naphthalin with an index of 1.66 would give a visibility of 22."—*The Microscope*, Antwerp, 1891.

This general rule applies not only to diatoms, but to all other bodies that the microscopist has occasion to examine by transmitted light.

The determination of the refractive indices of oils and other fluids is so readily and quickly accomplished by means of the Abbe Refractometer, that one is surprised that this instrument is so little known and used in the laboratories for the purpose of standardizing their various media. It is made by Zeiss and sold in this city by Eimer & Amend. The German price is 260 marks.

under 1.52.[1] A medium of higher index would bring the objects into sharper contrast. There are a number of essential oils that possess a higher index than cedar, and which can be used for this purpose, but I know of none that in this respect equals the oil of cassia. The oil is credited with an index of 1.605, but the index of the best sample that I have been able to obtain is 1.593. It is the product of the *cinnamomum cassia* Blume, and is the officinal *Oleum cinnamomi* of the United States Pharmacopœia. It must not be confounded with the true cinnamon oil (from the *Cinnamomum Zeylanicum* Breyne), as this latter has a much lower index. There is little danger, however, of sophistication in this direction, as the Ceylon oil costs eight or ten times as much as the cassia. Bacilli examined in cassia exhibit an unrivalled brilliance and sharpness of contour; and the minuter details, such as spores, flagella, etc., are shown with a distinctness impossible in cedar oil. The oil of cassia, like the oil of cloves, tends to abstract the color from bacilli stained with some of the aniline dyes, but not with sufficient rapidity to interfere with the diagnostic examination.

Dr. Bowles Lee, in his most excellent "Microtomist's Vade-Mecum" (London, 1890), in his chapter on clearing agents, gives a list compiled from Stieda, of oils more or less used by histologists. He also quotes the researches of Neelsen and Shiefferdecker, who examined and experimented with a large number of essential oils, and who specially recommend the oils of cedar-wood, origanum, and sandal-wood.

I find that there is a marked lack of uniformity among workers as to their employment of clearing oils, many

[1] Zeiss standardizes his at 1.515, but I have found a striking lack of uniformity in the oils from other lens-makers, most of them being lower than this standard. If adjustible objectives are used this makes little difference, as the lower index of the medium can be compensated for. If, however, the ordinary non-adjustable lenses are employed, the result will be an under-corrected, and consequently more or less untruthful, image.

preferring, for some reason or another, the oils of cloves, bergamot, lavender, cajuput, etc., and the confusion of ideas on the subject is rather embarrassing to the beginner. This is due in great measure, I think, to the fact that the name under which any given oil is sold is by no means conclusive either as to its identity or purity. The oil of origanum, for instance, is mentioned in every work on histology, and is found in every laboratory, and may be bought in open market at almost any price from thirty-five cents a pound to fifty cents an ounce. By the oil of origanum is meant the essential oil derived from the *Origanum vulgare* L. Desiring to obtain all possible information concerning this oil, I turned to Flückiger and Hanbury's Pharmacographia, and was surprised to find no mention of it in the index. Under another heading, however, I found the distinct statement that there is no oil of origanum in commerce. This Hanbury later confirms ("Science Papers," London, 1876), stating that the oil commonly sold under this name is in reality the red oil of thyme distilled in the south of France. Although it is exceedingly cheap, it is liable to adulteration with turpentine. Mr. Hanbury, however, desired an absolutely authentic specimen of the true oil of origanum, and had the plant gathered and distilled for him in England. Seventy pounds of the plant yielded but one ounce of oil, concerning which he says:

Contrasted with the oil of thyme the oil of origanum is distinguished by the following characteristics: 1. Odor, which is somewhat analogous to that of oil of peppermint, and entirely dissimilar from that of oil of thyme. 2. Color, which in oil of origanum is bright yellow, while the ordinary kind of oil of thyme is of a more or less deep reddish-brown.

It being quite clear that the substance sold as oil of origanum is not such, and it being equally certain that a good deal of the red oil of thyme is fraudulently sold under this name, the question may be asked whether there

are any other sophistications to be guarded against. Some time since I procured a sample of what I believed to be genuine red oil of thyme, and found its refractive index to be 1.483, and also found it exceedingly undesirable as to a clearing agent in the limited range of tissues in which I tried it. Besides this, it presented marked differences in refractive index, and in other particulars, from several samples of "oil of origanum" with which I compared it. The only conclusion, therefore, that I have been able to draw is that there are a variety of oils sold under this name, and there is no certainty that any two workers may be using the same identical substance. There is, however, an essential oil derived from the *Origanum creticum* L., which some writers say is the proper one to employ. The refractive index of a specimen of this oil in my possession I find to be 1.492.[1]

Lee very properly says:

It would be important to possess a list of the exact indices of refraction of the substances used for clearing. I have unfortunately not been able to obtain sufficient information of a trustworthy nature for the compilation of such a list. Cedar oil has nearly the index of crown glass [this is true of the oil in the thick state to which it has been brought by exposure to the air, not of the thin, raw oil, which is less highly refractive], it therefore clears to the same extent as Canada balsam. Clove oil has a much higher index, and therefore clears more than balsam. Turpentine, bergamot oil, and creosote have much lower indices, and therefore clear less.

I most thoroughly indorse Dr. Lee's recommendation, but would go farther and advise a thorough re-study of our clearing media. Let whoever will undertake this research first procure samples of the various essential oils of unquestioned genuineness and purity. Next determine their refractive indices. Then, commencing

[1]Dr. Delafield has recently given me a sample marked *Oleum origani cretici album*, which has an index of 1.479.

with those of either highest or lowest index, ascertain and record their behavior with celloidin and paraffin sections, both as regards the simple tissues (connective, elastic, epithelial, glandular, etc.), and the compound ones, such as the skin. The oils found most desirable should then be carefully described so as to insure subsequent indentification. Their color, consistence, specific gravity, and other physical peculiarities should be noted as well as their rotatory influence on polarized light. The determination of the refractive index is not alone sufficient for identification or test of purity; for instance, I have two specimens of cajuput oil that differ markedly in color, consistence, and behavior, and yet both possess the same index, namely, 1.465. It may be needless to say that the investigator should compare the data obtained by himself with the facts ascertained by prior workers, consulting freely existing literature, and particularly the writings of pharmacologists and physicists.

Whoever will undertake the study here outlined will without question deserve and receive the heartiest thanks of his co-workers in microscopy. The pathological thought of the times is exploring new avenues. In the days of the older pathologists we were content with the examination and study of morbid tissues in gross; later, with Virchow and his successors, we studied them more minutely, that is, the relation of the different tissues to each other. Now we seek to delve into the innermost recesses of the tissues themselves, and of the separate elements that compose them, and Metchnikoff has carried us still farther, even to the vital processes going on in the smaller living unicellular organisms. To investigate these to advantage one must avail himself of every optical aid that science places at his command.

DISCUSSION.

DR. H. S. STEARNS.—Several weeks ago, through the kindness of Dr. Piffard, I was enabled to make a number of tests with this new medium made of liquidambar, and the results were so extraordinarily good, that so far as my work in the Loomis laboratory is concerned, I expect to use nothing else in the future for mounting. By means of it I have been able to detect fine details of structure with an ordinary 1-6 inch objective, which could only be seen with difficulty with a 2 m. m. objective of 1.30 N. A. when the specimen was mounted in Canada balsam.

DR. J. A. FORDYCE.—I think that all of us, especially those who do micro-photography, will feel grateful to Dr. Piffard for his very useful improvements in the technique of such work. Many who do histological work have not the time, or do not take the time, to pay attention to those details which the author of the paper has so carefully worked out. Since using the Spencer lenses, with proper light and corrected in the way Dr. Piffard has explained, I have been able to obtain exceedingly satisfactory photographs of microscopical objects—much more satisfactory than I was ever able to obtain before. I find it of advantage in certain cases to vary the condenser according to the lens employed. The Zeiss apochromatic lenses can be used as condensers in combination with the Spencer objectives with the best results.

MR. C. F. COX.—I am very much interested in the general subject of Dr. Piffard's paper and particularly in the details which he has just presented. But the quotation which he read from Davis, with reference to the Abbe theory of diffraction, would be very discouraging to you physicians and histologists, if you were compelled to accept its statements as the end of the matter. It declares that unless your objective takes up and recombines all diffracted (as well as refracted) rays, you cannot get a truthful image of any object or structure composed of elements lying closer to one another than within the 1.2500 of an inch, and Professor Abbe has told us that no high power objective yet made is capable of so recombining all diffracted rays. If this is strictly true all these attempts to procure for us mounting media of high refrac-

tive indices and illuminating apparatus of wide apertures and delicate adjustments are practically in vain. In fact, a man cannot engage in refined investigations such as Dr. Piffard has described to us without, as it seems to me, largely discrediting the latest theory of microscopical vision. Dr. Piffard has not said whether he subscribes to the orthodox doctrine now in vogue, but for myself I am free to say that I have become accustomed to trust the high powers of the microscope further than the new theory seems to permit, and have come to believe that we see much more correctly than the later authorities are willing to allow.

I therefore agree with Dr. Piffard in thinking there is a great deal to be gained by the use of these very wide-angled lenses, and as an amateur I make bold to urge upon you professional gentlemen their more extended employment. The idea used to be that what we wanted most of all in an objective was penetration. There are still those who cannot get any satisfaction from an image that is not in bold relief, and they necessarily adhere to the old-fashioned, narrow-angled glasses. While within certain limits they are able to do very good work, they miss a great deal that might be accomplished by widening their angle, and at the same time broadening their mental view. By increasing the angle of the lens, and thereby lessening its penetration, you procure along with other desirable results, the separation of the object into thinner and thinner visual layers, and thus accomplish optically what is done mechanically by the microtome. This will often enable you to determine the character of a structure which you could not make out in any other way. If, for illustration, you had two different kinds of net-work laid one over the other, you might not be able to tell, under a narrow-angled glass, that they were separate layers. The penetrating power of the objective would be such that both nettings would be equally in view at the same time, and the resulting image would be a composite of the two structures. With a wide-angled lens, however, you could as it were pull one layer away from the other and see clearly its particular character. This is only one of the ways in which the newer objectives become useful to the histologist and the morphologist, and I speak of it because I think Dr. Piffard omitted this phase of the subject.

Dr. Piffard is entitled to our thanks for the work he is doing in the direction of increasing our means of refined and delicate observation. I may not entirely agree with him in his estimate of the value of wide-angled condensers, but on the matter of wide-angled objectives I think we are quite in accord, and I feel like seconding his efforts to extend the knowledge and use of these latest products of microscopical science.

DR. F. D. SKEEL.—I quite agree with Mr. Cox that we are indebted to Dr. Piffard for the efforts he is making to perfect the work of the microscope. The lens I ordinarily use in examining tissues and secretions for the tubercle bacilli is the one quarter inch lens, upon which I depend entirely. By means of it the bacilli can be seen perfectly clearly. Only a few years ago a story like that would be looked upon as moonshine, but I believe there are many here who use a similar lens for that purpose. I am inclined to ally myself with Mr. Cox in regard to the refraction theory of Abbe. I again wish to thank Dr. Piffard for his able paper.

MR. H. C. BENNET.—I have never used Dr. Piffard's medium of naphthalin-ambar in a histological way, but I have examined diatoms with it and for this purpose have found it much superior to the balsam medium. I also agree with Dr. Piffard as to the value of the wide-angled objectives.

DR. PIFFARD (closing the discussion).—I have very little to say in closing. I appreciate the points brought out by Mr. Cox, and almost the very expressions he used have occurred to me before. Science teaches that the highest aperture practicable in a given case should be used if we wish to differentiate structure. An object seen through a lens of low aperture may appear entirely different when seen through one of high aperature, although magnified to the same degree.

Stated Meeting, May 2d, 1895.

MEMORIAL SERVICES IN REMEMBRANCE OF THE LATE ALFRED L. LOOMIS, M. D., LL. D.

THE PRESIDENT.—*Fellows and Friends:* We are assembled this evening for the purpose of paying a tribute to the memory of one for whom we had a high regard. The tribute will be earnest, though inadequate; earnest, because of the great degree of respect and esteem we entertain for the memory of Dr. Loomis, but inadequate, because the present limits of the power of human expression make a greater attempt than this impossible.

It was deemed wise that the time of a stated meeting of the Academy be given for the purposes of this memorial, not only for the reason of the high station and great influence of Dr. Loomis in the councils and prosperity of the organization, but also in the hope that the lessons of the occasion would exercise a most potent influence on the memories and minds of the Fellows. Therefore, the hum of business, and the friendly, though spirited contentions on scientific subjects are silent now, if only for a brief time, that we may temper science with sentiment, sorrow with sympathy, and sadness with the solace of human friendship.

Those who are to address you this evening were friends of Dr. Loomis, and the associates in many of the earnest endeavors that characterized his useful and energetic life.

ADDRESS.

By LEWIS A. STIMSON, M. D.

Mr. President:

In the presence of this audience, containing so many who knew Dr. Loomis long and intimately, who were bound to him by the closest personal and professional ties, and whose hearts are filled with gratitude and affection for him, I feel again, and still more keenly, that sense of the inadequacy of my best effort to speak fittingly in his memory which was first aroused by the invitation of the Academy to take part on this occasion.

But that sense of the inadequacy of the spoken word is relieved by the reflection that the fullest and tenderest memorial of our lost friend and associate is to be found in those memories and emotions that rise in all our hearts as we gather here together to join in thoughts of him.

On the 23d of January, 1895, Dr. Alfred L. Loomis, for thirty-two years a member of this Academy, for eight years a member of its council, and for four years its president, died. And we have met this evening to do honor to his memory, to make public acknowledgment of our personal and collective obligations to him, and briefly to review his career, which gave lustre and dignity to our profession, whose activity for good spread far beyond professional limits to include in its beneficence the community as a whole, and which before it ended was crowned with all the public honors that the profession could place upon it.

Born at Bennington, Vt., in 1831, and left an orphan at an early age with only sufficient means to enable him to obtain a collegiate education at Union College, and a professional education at the College of Physicians and Sur-

geons in this city, Dr. Loomis, after having spent two years in residence in hospital, began practice here at the age of twenty-three years with no resources but his education and his mental, moral and intellectual powers, and even with those hampered by a body which already showed the early symptoms of that disease, pulmonary consumption, which had robbed him of his parents and all his immediate relatives. Aside from the physical weakness, this condition of limited means and narrow opportunities is an equipment with which we are all familiar, many of us by personal experience, and we know how powerful it is to stimulate to their highest activity the most estimable and valuable qualities, how rapidly it develops character, and how it protects from sloth and self-indulgence and from the insidious drift into a half-hearted search for and grasp of opportunity. Dr. Loomis's whole life has shown that he did not need this spur. The lad who, left practically to his own guidance, had voluntarily devoted his scanty means to obtaining an education; who, as a grown man, after the most brilliant success had been achieved, labored as persistently as he had done at the beginning, was evidently moved by some other motive than that of simply gaining a livelihood. That motive lay in a knowledge of his power and in an honorable ambition to give it full exercise and to win for it recognition. As an illustration of this I may be permitted to repeat a fine incident he once casually narrated to me with no thought of its bearing on his career. Sitting as a student on the benches in the lecture room and listening to one who stood at the head of the profession as well as at that of the faculty, he said to himself: "I will never rest until I stand in that man's place." And if the vow failed of literal fulfilment it was only because when the prize was fairly within reach it proved, like so many others that have blazed before the eyes of ambitious youth, much less worth having than it seemed. Other interests had arisen, other obligations had been

assumed, other rewards had been won, which outweighed it. The existence of this power, of the ambition which stimulated it, and of the resolute will which kept it in action was apparent to all. As one of his early contemporaries recently said: "Loomis was from the first looked upon as a man of great promise, and he was successful from the start. He was always at work, always energetic, and his energy was eminently practical."

Of medium height, slight figure, a rather dark face with keen piercing eyes, it is not difficult, with our later knowledge of him, to imagine his appearance at that time and to understand why he was looked upon by all as a youth of promise; nor is it less easy to understand why that promise was so promptly redeemed and in so full measure. A definite purpose to inspire him, an alert intelligence to guide, a tireless activity to execute, a will that never wavered, and a kindly, generous heart—such were the factors which brought success.

Specialization in medicine was at that time almost unknown here, and, like his fellows, Loomis took all that came in his way. He told me once with a smile that he had thought at one time he might do surgery, and that, backed by the presence and counsel of the late Dr. Van Buren, he had even amputated a limb. The experience, however, remained unique and within a very few years he was giving special attention to the diseases of the chest. Possibly the knowledge of his own inherited tendency to consumption which he fully appreciated, first turned his attention in this direction, but it seems probable that the explanation lies rather in the attractiveness which the art of auscultation and percussion, at that time developing so rapidly, must have had for one who so highly valued precision and accuracy in diagnosis. His advance was so rapid, his utilization of his opportunities so vigorous, that within a very few years he was at the head of the largest private class of students in the city; one that competed so

successfully with the college quizzes that in 1862 he was appointed lecturer upon physical diagnosis in the College of Physicians and Surgeons.

In the meantime his health had broken down, and his anticipation that he would not survive beyond the age of thirty years seemed about to be realized. He left the city, sought the heart of the Adirondacks, remained there for six months, and regained his health. Warned and instructed by this experience, he thereafter spent two months of every year in the same health-giving environment.

In 1864 began his connection with the University of the City of New York, in the fortunes and development of which, as a member of its medical faculty and subsequently as one of its council, he was to take so important a part. Of this part another will speak to you this evening, with more authority and in more detail. After having served for two years as adjunct professor, he succeeded to the full chair of the practice of medicine on the retirement of Dr. Metcalfe in 1866. The fortunes of the school were then at a low ebb. As it had drawn its students largely from the southern states, its classes had been greatly reduced by the war, and in the summer of 1866 its building in Fourteenth street had been destroyed by fire. This was followed by the resignation of several of its most prominent teachers, and it found itself without a home, without money, and almost without students. It is sufficient to say here, in this record of Dr. Loomis's life, that in five years he had become its virtual head, ten years later it had reached the highest pitch of prosperity it had ever known, and that five years ago it received through him from an anonymous benefactor, gifts amounting to nearly half a million dollars by which the Loomis Laboratory has been built and endowed, the college property has been cleared of debt and placed in the hands of a board of trustees, and the school has been made in fact, what it was previously only in name, an integral part of the University, free of

any proprietary interest vesting in its faculty. It is only my plain duty, as a member of that faculty, to testify that this was almost wholly Dr. Loomis's work. It was his energy, his reputation, his purse that sustained the school during its dark days; his judgment guided it; his influence brought to it all the varied aid it needed; his skill as a teacher, his reputation as a physician, filled its benches; his deep interest in the advancement of the profession raised its standards, graded its courses and improved its methods; and, finally, it was through the gratitude and affection that he had inspired, and solely as a tribute to him individually, that the magnificent gift was made which placed it forever upon a secure foundation.

Such an accomplishment, with all its varied and far-reaching results, would confer on any man a title to the respect and gratitude of his fellows, and might well be deemed the ample fruit of a life's work; but Dr. Loomis found time and opportunity to accomplish similar results in many other fields.

Appointed attending physician to Bellevue Hospital in 1859 and to the Charity Hospital a few years later, he remained in the service of the Department of Charities and Correction until his death. This service was not limited to the care of the sick under his charge; it was broadened to include the interests of all—the Department, the hospitals, the sick, and the profession. He stood aside from none of the many questions and struggles that arose, but took a leading and often a controlling part in all. With that long series of changes we are all familiar; the vast improvement of the grounds and buildings, the introduction of trained nursing and improved methods, the equalization of representation on the staff, and ultimately its systematic division which for fifteen years has given us the best service Bellevue has ever had, have kept us free from the worries and unseemly "politics" of the ever-recurring strife for place, and have so greatly favored the regular

and systematic use of its clinical facilities for the instruction of students. Not only within those hospitals with which he was directly connected was his influence felt. Though unseen and unofficial, it was powerful in many others through the counsel sought from him by their managers and through the power of his support given to applicants for places in them. Probably no other member of the profession was so frequently asked to aid by his recommendation or his active support, candidates for hospital and dispensary positions, and with good reason, for his support was not formal and perfunctory; it was active and earnest. He made the applicant's cause his own, and fully shared his disappointment in defeat if not his elation in success. Many of those here present will feel again at these words the gratitude they felt, years ago perhaps, for the powerful aid so heartily given them.

In the founding and management of two other hospitals he took a leading part: the Sanitarium at Saranac in the Adirondacks, and the Hospital for Consumptives at Liberty. Each had its origin in the desire to extend to the poor the facilities for restoration to health which heretofore were within the reach only of the rich. Profoundly convinced of the curative influence of a residence in the Adirondacks for those affected with phthisis, he joined with Dr. Trudeau in establishing at Saranac, buildings at which consumptives might live with comfort and even with some luxury at moderate prices, and they made provision to hold a certain number of rooms for the very poor. Mrs. Loomis gave one of the cottages, and thrice every year he visited and examined all the patients, and habitually gave his services here in New York to examine and pass upon the claims and needs of those who sought to profit by the charity.

The other institution, the Hospital and Dispensary for Consumptives, was begun only a short time before his death, but since then the generous gift by Mr. Morgan of

$60,000, has ensured the construction of the main building, to be known as the "Loomis Memorial Hospital," at Liberty, in Sullivan county. It was planned for the help of those for whom even the railway fare to Saranac and the low price of residence there were beyond their means; and an important part of the plan is the dispensary in this city at which nutritious food, cod-liver oil, and medicine are supplied to those who cannot even intermit their work while seeking a cure.

In society work also Dr. Loomis was habitually active and prominent, both in the professional discussions and in direction and management. He was elected to the presidency of every society of which he was a member—to name only the more prominent—the State society, the Academy, the Climatological, the Association of American Physicians, and, finally, of the Third Congress of American Physicians and Surgeons in 1894. There was a feature in his selection for these positions that makes it especially noteworthy, namely, that in every case the honor came to him unsought and by the unanimous vote of his fellows. We all know, though we may sometimes lose sight of the fact, that no honorable position confers honor upon an unworthy holder or upon one who has gained it by unworthy means. The man who deserves it has little to gain from it beyond the gratification afforded by the public recognition of his worth. For him who does not deserve it or who has obtained it by unseemly means, it serves only to make the unfitness more apparent. The honor comes from the recognition of worth. Dr. Loomis was far too clear-sighted to have any illusions in such a matter. On the few occasions on which I spoke with him about his possible election to an office, his answer always was: "I should be gratified to be chosen, but I will enter upon no contest for it. I will not even be a candidate if there is another one." On some occasions he was chosen because the honor was simply a fitting acknowledgment of

his position in the profession; on other occasions because there was special work to be done and he was deemed the best fitted to do it. The choice was always honorable to him, and as such it deserves to be recorded.

He became a member of this Academy in 1863. In 1885 he was made one of its trustees, and shortly afterwards when its work was reorganized and its sections created, he gave his personal influence and efforts to the success of that reorganization by taking the chairmanship of the Section of Practice. From that time until his death he was unremitting in his labor to increase its influence, to raise its standards, and to advance its material and professional prosperity. During his trusteeship the idea of securing a suitable permanent building began to take shape. It was furthered by two large gifts, one from Mrs. Hosack, the other from Mrs. Woerishoffer, and in 1889 Dr. Loomis was urged to take the presidency and complete the work. How vigorously he entered upon the task, how successfully he accomplished it, is known to you all. He brought to it all his powers of persuasion, all his energy, all his influence with the profession and with the laity. I have been assured by those who were associated with him in the work, that to him, far more than to any other, is due the final and brilliant success. As with everything else he undertook, the work was ever on his mind. He was instant in season and out of season. No opportunity to advance it passed unimproved. His subscription book lay on his office table, and no patient who was worth the effort failed of an opportunity to place his name upon it. His enthusiasm and interest kindled those of others. Two individual subscriptions obtained by him amounted to one-fourth of all that was raised, and I have been assured that more than half the entire amount was obtained directly through his personal efforts. The indirect results cannot be measured. They are to be found on all sides, brought about by the enthusiasm for the object

he inspired in some, and by the confidence and affection felt for him by others which led them to make his interests their own and to give freely of their time and labor. Indeed this building, in which we are gathered to-night in his memory, is itself a monument to him, a reminder of all that this Academy owes to him.

Nor was it in the advancement of the material interests alone of the Academy that his influence was felt here. His views concerning its duties to itself, to medical science, and to the community, were broad and well thought out, and the four years of his presidency saw a steady advance in the direction in which the Academy had begun to move under the guidance of his more recent predecessors. His first inaugural address, upon "The Present Needs of Scientific Medicine in this City," was an appeal to the profession to urge the entrance into it of youths of promise, and to dissuade from it those who are "devoid of natural talents, mental discipline, and intellectual power," to the colleges to raise the standard of their teaching and requirements, and to the public to support the colleges and laboratories by pecuniary aid. He called attention also to the need of a higher standard of medical literature and pointed out the means by which it was to be obtained, and finally called upon the Academy to create and maintain "an ideal medical spirit in the profession of this city and our country." His speech upon the occasion of the formal opening of this building and his second inaugural address breathed the same spirit, and appealed to the same ideals. In no address delivered before this Academy was a higher note ever struck, a loftier spirit ever shown, a more stimulating call to duty ever made.

He also sought to promote sociability and acquaintanceship among us by establishing, for a time out of his own purse, those social reunions that follow our formal meetings, and he left to the Academy by his will a fund for their support in the future. In short, his influence and

his efforts here were steadily directed toward higher ideals, greater personal and collective usefulness, and a broader, kindlier spirit of companionship and association.

Such is the record of this active and beneficent life. It remains only to speak briefly of his relations with his patients, and to review the mental and moral qualities that made him what he was.

The relations between patients and their physicians are usually characterized by much confidence and affection. With Dr. Loomis this was the case to a singular degree, and probably the death of no other member of our profession, within our time, has caused so widespread personal grief as well as the sense of the almost irreparable loss of a protector in the gravest emergencies of life. Dr. Loomis's clientele was drawn from every city, from almost every town, in our broad country. His name was almost as well known in Chicago and in San Francisco as in New York, and those in New York who did not know it were few. Those who consulted him believed in him with a faith that was never shaken, and their confidence in him extended far beyond professional lines. He was trusted not solely as a physician, but as a man; and he inspired confidence and affection even in hearts that are slow to yield to the gentler emotions and in heads that habitually and successfully look only to themselves for guidance. And with no suggestion of disrespect for the many noble men who are doing similar work to-day, I think all will agree with me in saying that few men in this city—certainly no other in our profession—exerted so powerful an influence in so many important directions. Through him men who had no other points of contact met and combined in beneficent activity; and his death, by destroying for the present that facility of concentration of effort, causes a definite loss of efficiency in forces that work for the good of the community.

The source of this influence is to be found not solely in

his professional acquirements and acumen, great as they were, but rather in his force of character, his directness of method, and in the elevation and integrity of his purposes.

I have said that he was a tireless worker, but that does not express it all. He was at work all the time. In addition to the constant demands of a large practice, he was never without some large, definite purpose that seriously occupied his thoughts. This made him a serious man; serious in mind, serious in manner, jealous of his time, eager to seize opportunity, determined to learn the exact and entire truth. It seemed to me at times that he looked upon every new case as a challenge of his ability, of his reputation. Many of them, indeed, were such, for they came to him after consultation with others. They consequently received his concentrated attention, and that, aided by his long and accurate training, rarely allowed any important fact to pass undetected or underestimated. Still more striking was the certainty with which he seized upon the fundamental points in the case, passing by others which were more prominent, but misleading. And, above all, he had that quality, which distinguishes all great clinicians, which makes them healers of men, not merely students of disease—the ability to consider and treat the patient as well as the disease. Many another might recognize as accurately the condition of a lung, a heart, or a kidney; our shelves are filled with books that formulate the corresponding treatment, but his eye, his mind, saw beyond the morbid process to the man who was fighting with it for his life, and brought to him such help as could be given. It was this that gave his great resourcefulness so large a field, this that made him so valuable and so much sought for in consultation.

Dr. Loomis's character was further marked by great practicality and directness. He was not, however, simply a fine executive, one whose highest efficiency is found in carrying out the ideas of others. On the contrary, he was

essentially a man of ideas, a man who did his own thinking. But he was not an idealogue, not a sophist, not one to spend his time in idle contemplation, nor one to waste effort along lines that could lead to no definite result. While he may have had but little of that quality which was esteemed among the highest virtues by the old moralists, the passionless wisdom (Σωφροσύνη) of the philosophic Greek, the *Temperantia* of Cicero, the *Moderation* of St. Paul, he was wholly free from that other which so often masquerades in its form, which hides mental and moral irresolution and weakness under the cloak of deliberation, the quality which, when unbalanced by strong practicality and resolute will, keeps a man so alive to the force of what can be said in favor of the other side that a clean-cut, strongly held decision becomes an impossibility for him, and which finally lands him as a leader in the quagmire of impotent compromise. Of that quality he had nothing. His convictions were clear and strongly held, and when the time came he never hesitated to express them uncompromisingly, and never flinched from word or act that seemed required by them.

To every question he gave at the outset, earnest, concentrated attention; he possessed himself of all available information, promptly reached a decision, and then all his thought, all his energy, were given to embodying that decision in action, unhampered by the persistence of any doubt or misgiving. In a weaker man such a course would lead to great disaster; but his preliminary consideration was exhaustive, and he was not a man to believe a thing to be because he wished it to be, or to take a preconceived idea and twist all facts to suit it. To those who saw only the firmness with which he held his opinions and the energy with which he sought to enforce them, he may have seemed narrow and prejudiced, but those who knew him more closely, those who had the opportunity to learn on how much careful thought and observation those opin-

ions were based, could not fail to appreciate the pains he took to be right, his breadth of view, and his calmness of judgment.

To this thoroughness of study and preparation was added great alertness to perceive and grasp opportunity, and great activity in profiting by it. His life shows no special favoring of fortune; on the contrary, he had more to contend with than most of us have, and his opportunities were only such as come to all of us. His success came from his readiness and his ability to use those opportunities, and to him may fitly be applied Tennyson's description of the successful man as one who

> "breaks his birth's invidious bar,
> And grasps the skirts of happy chance,
> And breasts the blows of circumstance,
> And grapples with his evil star."

His manner was reserved, almost cold, but this was the result not of any lack of sympathy, but rather of his habitual caution about speaking before he was ready to speak; and readiness to speak came only after he had reached a conclusion by which he was willing to stand. He cherished most jealously his respect for his word, whether it was given as an expression of opinion, as a statement of fact, or as a promise, and therefore he was slow to utter it.

Although he held himself so well in hand, he had a fiery spirit, a glowing nature, which at times broke sharply through the artificial restraints of prudence and caution and found full expression in work and deed. It was this spirit and the power of imagination and intensity of feeling that accompanied it which made him appear so differently in his public addresses, and especially in extemporaneous speaking on important occasions and before sympathetic audiences. The transformation at such times was complete, and he who ordinarily was slow, even hesitating, in speech and often strangely infelicitous in phrase, became fluent, graphic, persuasive and inspiring.

His reserve and self-restraint, although so sedulously cultivated throughout his life that they grew to seem an integral part of him, were yet only a screen behind which were the live coals of a nature glowing with interest in all about him, keenly alive to all questions of public or private concern, solicitous of approval, warmly affectionate, and emotional to an extent that was at times childlike in its expansion and ingenuousness.

Uncompromising and persistent as he was in the pursuit of an object, and while he would abate no jot of a principle, he could always be reached and moved in personal matters by an appeal, and he was always ready to hold out his hand in friendship or in help to an opponent whom he respected. For this reason and because his objects in all public matters were untainted by personal private interest, he received the respect and hearty liking of most of those with whom he had hotly contended, and they were always able and willing subsequently to co-operate with him on other issues. These were the qualities, too, that bound him so closely to his friends; for he gave them not merely help in their need, but also a warm and hearty sympathy and confidence at all times. A striking illustration of the strength of the bond between him and his friends was given during the three days that he lay dying, when his door was thronged with men whose names are among the highest in financial, commercial, and professional circles, asking for news of his condition and begging to be allowed to do something for him, if only the humble errand of a messenger.

Death took him in the plenitude of his powers and at a time when his usefulness was expanding in new and important channels in which he had had only time to show how much was to be expected from him. It came with a suddenness that recalls the cry of the dying Frankish king: "Who is this mighty power that pulls down the strongest?" It has taken from the profession one who

gave it strength, dignity, and respect; from the community one whose attention was always alert to its interests and whose influence was powerfully, if quietly, exerted in its behalf; and from his friends one whose place in their hearts can never be filled.

ADDRESS.

By HENRY M. MacCRACKEN, D. D., LL. D.

Mr. President: For more than thirty years Dr. Loomis wrought for university education, his sole field throughout this period being the New York University. I count it a privilege and an honor to be called, as Chancellor of this University, to pay our tribute to his greatness. In attempting to estimate Dr. Loomis as a teacher, I feel my lack of at least one qualification, which may be claimed by three thousand of my fellow-citizens, namely, the experience of a pupil in his class room. Not less than three thousand physicians have received Dr.. Loomis's instruction, and know by this experience how great he was as a teacher. Nevertheless, for more than a decade I have been his associate in the University Faculty, and have continually sought to find his measure and the secret of his strength. I have found it first in the divine vocation which came to him to give from his treasures freely to others. Some men are givers by nature, some by the grace of God, and some by both. Dr. Loomis was a giver by both. The true teacher gives because he must give. He says with Chrysostom:

> "I speak like as the fountains bubble;
> I teach like as the rivers flow."

Dr. Loomis's oldest associate in office says that as he entered Bellevue Hospital one day, a gentleman stepped into the hall and, turning to five or six young men who were standing there, said: "I propose to give some clinical teaching at the bedside, and would be glad to have you come in." When he inquired who the gentleman was, he was told it was Dr. Loomis, a member of the staff of the Hospital, giving instructive lessons in examinations of the chest for diseases of the heart and lungs. These lessons

were the precursors of a quiz class, which began with three students, but rapidly increased. This incident of thirty-two years ago shows the inevitable teacher.

The spirit of giving which impels the teacher to teach, leaves him many times quite like other men in respect to giving anything besides knowledge. It was not so with Dr. Loomis. I know not the particulars of the labor and money expended by Dr. Loomis throughout thirty years for the building up of the University medical faculty. He did not speak often of what he had done, nor did I ever hear him speak of it at all, except when it seemed necessary to enforce and establish some important measure. In his speech at the Stuart meeting of four years ago, spoken at my invitation, to influence our citizens to join the University in a great aggressive movement, he made the following reference to his own experience upon the business side of medical education. He said:

"It is not because I have received money from it that I have taught in the University for twenty years; I have not received pay enough that any one of you would work six months for, for the whole time—I have given almost as much as I have received for it; but it is because I am interested in the University, and I want you to be interested in it."

During the first eight years of my acquaintance with Dr. Loomis, his efforts for the University were confined almost wholly to the building up of the Faculty of Medicine. The work to be done was vast, and there were few to perform it. None the less, he was always ready with counsel and suggestions when I approached him in regard to the interests of the University at large, or the work of our other faculties. From the beginning of our new movement in 1891, he threw himself into the general university work with increased interest. He was able to do so because by that time he had led the medical school out into a large place. The Loomis Laboratory had been builded. A generous friend of higher education, and also of Dr. Loomis, had

planned the enlargement of our university work in medicine, and made large provisions for the carrying out of his plans. Virtually a second foundation of our college of medicine was accomplished in five years, from 1887 to 1892. The property and endowment for the use of the faculty of medicine, including laboratories, were increased from threefold to fivefold within this quin-quennium. One feature that characterized this remarkable movement is unparalleled in my knowledge of educational work—not only was there no announcement authorized of the benefactor or benefactors, but there was even a withholding of any formal announcement of the benefaction. The motto of this movement was St. Paul's motto: "Every man's work shall be made manifest; the day shall declare it." If good fruits spring from this movement and these benefactions, they will be seen in the advanced training and education given by the university to doctors of medicine.

From this study of how Dr. Loomis put his heart into our university work, I turn to consider how he put his intellect into it as well. His was no common intellect. Its presentative power was quick and keen; its memory was strong; he had no little constructive imagination. The greatest strength of his intellect, however, I would place in his judgment or thought power, his power to see, to compare, to classify certain facts placed before him, and rapidly to make important inductions therefrom. Often he performed the process so quickly that it seemed like intuition instead of reasoning. It was the same strong reasoning power whether applied to small things or great.

The last note which he wrote me was a few hours before his mortal stroke. When I received it he was already in the Valley of the Shadow. It stated in half a page his judgment on the choice of a university mechanician, founded on a half-hour's examination of a candidate and his credentials. I have never met a man who made fewer mistakes. These tributes of brain which I am naming are

alike valuable in university work, whether in teaching or administering.

I know not how much injury has been done to education by the supposition that, granted a man great in memory and accumulation of knowledge, he may be a professor of high value, even though in his power of reasoning he be very weak. What has led to this is, of course, the evident need of extended and special knowledge for every one who would fill ably a university chair. But we have kept the importance of knowledge so largely before our minds that we have allowed ourselves to forget the equal importance of wisdom. The same swift, true judgment that made Lincoln a statesman would have been essential to him if he had resolved to be a university professor. Dr. Loomis had the Lincoln-like quality of disencumbering himself, his subject, and the attention of his hearers of unimportant elements, and concentrating attention upon the great questions involved. This quality he manifested in teaching. He evinced equally the same quality when he was seeking to carry others with him in some matter of administration or ordinary university business. More than one of his pupils has said to me that the chief characteristics of Dr. Loomis's lectures were their clearness and conciseness. In drawing a picture of the disease, he brought out the strong points, "pegs," as he expressed it, "on which to hang something." He would describe as nearly as possible a typical case, and did not lead the mind astray by giving all the possible exceptions. He also impressed the students that he believed what he was saying. As a clinical teacher at the bedside of the sick, he was thorough, positive, and helpful. The force and determination of his character were brought out in his lectures, as in everything else. Right here let me say that what we call determination of character leads us at times to deprecate or overlook intellectual qualities which exist alongside this quality of will. Will is like the electric force in the machine, but the

movement of the electricity makes it no less important what kind of a machine you have. Your greatest printing press surpasses the press of a century ago, not merely because it has that immense electric power to move it, but because in construction it transcends the simpler instrument. Dr. Loomis's students felt his strong will, but they believed no less in his accurate, true intellect. I need not say that these qualities of intellect forbade him to ride hobbies, whether in his work as a professor or director. To thinking men he could never become tiresome or dull.

Among the other results at which his intellect arrived was his Christian conviction. He accepted the Father God and the supernatural revelation of Christ. It is notable how among strong men those who exercise constantly their reasoning powers arrive at Christian convictions in larger proportion than men distinguished chiefly for mere accumulation of scientific facts. When a neighbor of mine, a chief justice of the United States, in the very meridian of his intellect, made public profession of Christian faith, I was led to observe that he was simply one in a line of predecessors from Chief Justice Marshall to Chief Justice Chase, who, as logicians, had gone the same path. I scout the heresy that Christian faith depends slightly on logic. From men of little logic, it is true it requires but little logic; from men of great logic, it demands all the logic they possess. Dr. Loomis's logic was in his Christianity.

This brings me once more to dwell upon his moral qualities, which were clearer and nobler because of his power of will. He consented to humbleness of labor in order to do good. His clear mind showed him that care of details was essential to usefulness. He took the place of a worker on the university executive committee; and in his three years of membership, none proved more punctual to the hour, none more patient amid wearisome details, none more persevering in convincing our citizens that a man

should not live by bread alone, but by deeds of beneficence for their generation. More difficult than all else, he was ready to resign his laboriously formed plans, and adopt the plan of another just so far and so fast as any point of superiority was made manifest to his own mind or to the minds of his peers. His associates in the medical college will join with his associates in the university council to testify that he was as cheerful and prompt when going abreast as when he was in front. Not only did he often put his own plans to one side for the sake of the plans of others, but he constantly saw, beyond and above his own best plan, even when it was fully accepted by others, the distinct outlines of an ideal, for the sake of which he ever held himself ready to subordinate past attainment.

One of his nearest friends said to me at his burial: "Dr. Loomis had plans for education five years ahead." My study of Dr. Loomis convinces me that he had, in one sense, plans ahead for not merely five years, but five generations. This, again, is the characteristic of a soul conscious of infinity. It says: "Not as though I had already attained, but I press forward." Nothing makes Dr. Loomis's loss to the university more felt by us than this last named attribute of his character.

Thorwaldsen, the sculptor, was found, it is said, in utmost depression one day, and gave as the explanation that he felt perfectly satisfied with the latest works which his chisel had wrought. It warned him that the future promised him very little. But who of us ever saw Alfred Loomis in serious depression? His was still the motto of Goethe, "Ohne Hast, Ohne Rast." He did not reach the land of his ideal, therefore he did not reach the realm either of intellectual or volitional repose. This explains why he seems still living to us this hour.

The force that was embodied in the form now reposing in Woodlawn is, and will be, a deeply felt force in the New York University.

ADDRESS.

By ABRAM S. HEWITT.

Mr. President: Although I an painfully conscious of my inability to give expression to the admiration and the affection which I had for Dr. Loomis, I have to express my thanks to you and your associates of the Academy of Medicine for the opportunity to participate in these exercises in memory of one of the most eminent physicians of our day and generation. The graceful and touching tribute of Dr. Stimson, to which we have listened to-night, relieves me from any further reference to the professional career of Dr. Loomis. His labors, services, and sacrifices in behalf of medical education and of the diffusion of useful knowledge generally have received a well deserved and eloquent recognition from Chancellor MacCracken, whose peculiar fitness for this labor of love all will recognize and gratefully acknowledge.

But Dr. Loomis was more than a physician, a teacher and a friend of education. He was a good citizen, who recognized and performed his duty to his fellow-men as members and co-workers in the same social organization. He understood that society exists for the maintenance of public order, that order rests on law, and law must be based on justice. Hence in a government deriving its authority from the will of the people, it is the duty of every citizen to take part in the choice of public servants, to do his share in securing the enactment of just legislation, and in its enforcement in the face of opposition, and against the clamor of ignorance and selfish interests. To perform this duty requires conscience, self-sacrifice, and courage.

It was in this sphere that Dr. Loomis showed the un-

bending rectitude and the patriotic devotion of his character. I think I shall best respond to the affectionate feeling which has called together this assembly of his friends and admirers by a simple narrative of the circumstances which brought me into close relations with Dr. Loomis, who, at the time when I became mayor of the city, was a trustee as he was later the president of the Academy of Medicine.

On taking office I found that the health department of the city was in a state of utter demoralization. The president of the board had been removed by my predecessor for malfeasance, but as the removal had not been acted upon by the governor, as required by law, he was still a public official responsible for the sanitary condition of the city. The medical member of the health board was already stricken with that mortal disease which in a few months terminated in his death.

Next to the police, the health of a great city is the most important department of its government. The city of New York would become uninhabitable and its wealth would be dissipated in the presence of a pestilence caused by the failure to enforce the sanitary regulations rendered indispensable by the existence of a dense population. While the interests involved are vast and varied, the questions to be answered are technical. In the hands of inefficient or incompetent officials the health department becomes a source of danger instead of a shield of protection. Such was the crisis which confronted me. It seemed to me that my first and main duty was to rescue the city from the danger to which it was exposed by the chaotic condition in which the board of health was found to be. I placed myself in immediate consultation with the officers of the Academy, and by its advice and influence the approval of the governor was secured and the office of president thus made vacant. With much difficulty a competent and honest man was found, who, although without political backing, was promptly appointed to the vacant position. His ad-

ministration justified his selection. The next step was to secure the services of a competent physician to take the place on the board of health made vacant by the death of Dr. Woolsey Johnson. You, sir, with considerable reluctance and at much personal sacrifice, consented to accept the position, and thus the department was restored to the confidence of the public and of the medical profession. I need not enlarge upon the admirable results which were thus achieved, and in which you would be fully justified in saying,

"Quorum magna pars fui."

Now, during this period of painful anxiety, Dr. Loomis was my sheet anchor. He was at my call day and night, and fortified by his advice and approval, I felt that every step had been taken upon safe ground. It was then that I learned to appreciate the clear mind, the firm will, and the spirit of self-sacrifice which made him the man he was—honest, able, patriotic and unselfish in the public interest.

The organization thus effected was just in time to provide for the advance of cholera, which was on its desolating path from the Orient to the West, and had already penetrated to the eastern confines of Europe. I discovered that the quarantine establishment was in a state of dilapidation which, by comparison, made the demoralization of the health department quite respectable. The Academy of Medicine was again applied to for investigation and advice. Upon my request a committee of very able and eminent physicians was appointed to recommend measures necessary for the protection of the port from the impending pestilence. On this committee Dr. Loomis and Dr. Agnew were especially conspicuous and active. I mention their names only because they have ceased from their labors, but I was profoundly impressed with the devotion of the whole committee to the unpaid task to which they had been

suddenly summoned. In the presence of such public spirit I felt the force of the old Roman battle cry:

"De Republica nil desperandum."

The report of the committee was a monument of exhaustive investigation. Its recommendations were clear and unassailable. A bill was prepared appropriating $200,000 for the immediate needs of the establishment and forwarded to the governor, with a letter from the mayor, stating therein the dangers to which the city was exposed unless the recommendations of the committee were carried into effect without delay. The governor acted promptly, and the legislature lost no time in passing the act, making the required appropriation. The quarantine establishment was thus put in order, and although the cholera came in 1888, as was expected, not a single case in that year reached the city of New York, which was saved from the horrors of pestilence and the ruin which would have followed the general suspension of business. To Dr. Loomis chiefly, aided by his associates of the Academy, was due the immunity from a calamity, the extent of which cannot at this day be properly estimated.

I narrate these occurrences because I was thus first brought into intimate personal relations with Dr. Loomis, ending in a friendship which terminated only with his death. I then learned to know the other side of his remarkable personality. As the physician and the public man he was apparently reserved, stern and unbending. He was not a man of many words, but no one could mistake his meaning. His conclusions were positive, because they were the result of careful examination and profound thought. He was not a man to let "I dare not" wait upon "I will." He had the courage of his convictions, and what was more important he had the faculty of impressing upon others the absolute truth of his conclusions. Hence he was a man of uncommon influence, and from his sin-

cere and earnest nature became naturally a leader of men. It is, therefore, not to be wondered at that in every association with which he was connected he became, sooner or later, its chief executive officer. His advice was sought by men eminent in other walks of life upon matters of general or of special interest not connected with his professional career. Hence, among his friends he numbered the leaders in the world of finance and in the domain of industry. It was the confidence thus felt in his judgment and his stern integrity which enabled him to secure the munificent endowments of which Dr. Stimson and Chancellor MacCracken have given us some account. No doubt the donors were induced to feel an interest in the enterprises to which they were thus committed, but I am sure in every case that the gift was a tribute rather to the personal character of Dr. Loomis than to the undertaking which he recommended. He thus became responsible to them and to the world for the success of the work in which he engaged. He accepted the responsibility with a willing heart, and I am persuaded that I only give voice to the judgment of those who entrusted to him their means when I affirm that in no instance was their confidence misplaced, nor did the results fail to justify the expectations and representations of Dr. Loomis.

Even a layman cannot fail to remark the advance, almost amounting to a revolution, which the present century records in the practice of medicine. In pulmonary diseases especially, not only new methods of diagnosis, but new systems of treatment and novel remedies have been found. The world of bacteriology has been discovered and been partially explored. How far Dr. Loomis has contributed to this progress and laid the foundation of future developments is better known to the profession than to me; but I know enough to assert that his name and fame must ever be associated with the evolution and discoveries which will make our day and generation memor-

able for the alleviation and prevention of consumption, justly regarded as the scourge of the human race. In these labors Dr. Loomis has justified in our day the pardonable boast, once made by Sir Benjamin Brodie, "It may be said of our profession that, unlike most others, it has for its constant and sole object to confer benefit on others. The advocate at one time pleads for the guilty, and at another endeavors to convict the innocent. The soldier engages to go wherever he is sent that he may destroy the lives of those from whom he never received an injury; but the physician and surgeon are engaged only in lessening the afflictions and prolonging the existence of their fellow-creatures."

But it was in the intimate intercourse of personal friendship that Dr. Loomis shone in a new and more attractive light. His apparent reserve of manner disappeared, and it seemed to give him delight to exchange the position of the teacher for that of the learner. The wide range of his sympathies and tastes was made manifest. Occupied as he was to the full measure with the stern realities of life, he seemed to find relief and recreation in the consideration of abstract questions bearing upon social and political development. I found him ready not only to discuss the principles of human action, but profoundly interested in their application to the intercourse of men and nations. But it was chiefly in the domain of morals and religion that his soul seemed to find its chief delight, and in the presence of nature it expanded into positive joy. Doubtless his fondness for the free life of the Adirondack wilderness may be thus explained, and the vacations of successive years passed in the solitude of its woods and waters may have contributed largely to the habit of introspection, which is characteristic of a religious temperament. He undoubtedly loved solitude, and Antæus-like derived new strength from each fresh embrace of his mother earth.

It was my good fortune to become his friend at the time

when he had decided to build for himself a home in the country, to which he might retire from the activities of professional and civic life when advancing years should demand rest and recreation. He told me he wanted air, sunshine, pure water, isolation from the contact with business and near neighbors, out of sight of railways, but still near enough for convenient use, and above all a wide view of the mountains and forests which, as will be seen, were to his mind the object-lessons revealing the divine power of his Creator.

He seemed to feel with Longfellow:

> "If thou art worn and hard beset
> With sorrows that thou wouldst forget,
> If thou wouldst read a lesson that will keep
> Thy heart from fainting and thy soul from sleep,
> Go to the woods and hills! No tears
> Dim the sweet look that Nature wears."

Such a site it was my privilege to show him in the Highland range of mountains stretching from the Hudson to the Delaware river. I shall never forget the delight with which he took his first view of the lovely lake and the mountain side upon which he subsequently erected a charming residence, where for a brief period he was permitted to realize the pleasures of an ideal home in the midst of his family, in the companionship of his friends, and in the outlook upon a landscape which is without a rival on the Atlantic slope of the Appalachian chain. Here the rounded mountain peaks, like the waves of a tumultuous sea, seem to be rolling upon each other in endless succession. The vast forest stretches as far as the eye can reach—in winter, leafless; in spring, glorious with blossoms of every hue; in summer, clad with the garniture of verdant foliage; and in autumn, all radiant with the varied hues of ripening and decaying nature. In the presence of this inspiring scene the spirit of Dr. Loomis seemed to undergo an entire transformation. I recall the intense de-

light with which one day in May he beheld from his wind-capped mountain, the sudden awakening from the sleep of winter and the new birth into life and color of the face of Nature. It was the ever-recurring miracle which proclaims the reign of law as well as the existence of the Lawgiver. To Dr. Loomis it was a revelation of beauty and goodness which found a reflection in his expressive features, and in the exuberance of his joy he seemed to have renewed his youth and to be in love and charity with all mankind.

> "For him there was an eloquent voice in all
> The sylvan pomp of woods, the golden sun,
> The flowers, the leaves, the river on its way,
> Blue skies and silver clouds and gentle winds,
> The swelling upland, when the sidelong sun
> Aslant the wooded slope at evening goes,
> Groves through whose broken roof the sky looks in,
> Mountain and shattered cliff and sunny vale,
> The distant lake, fountains and mighty trees,
> In many a lazy syllable repeating
> Their old poetic legends to the wind."

In such a nature as was his, it is scarcely necessary to say that there was and could be no conflict between faith and reason. Each had its appropriate sphere, and there was no interference of the confines. His careful study of facts, his profound love of nature, his sturdy and earnest pursuit of truth, never led him to Pantheism in explanation of the mystery of life and the order of the universe. To him the Creator of all was a personal Being, just as his own soul was an individual essence which could never be extinguished. He had no theological doubts and he was not disturbed by the Unknowable; but the simplicity of his character led him directly to the Saviour as the true and only Mediator between God and man.

I trust that I do not overstep the bounds of propriety in this personal relation, but it seems to me that no adequate idea can be formed of the noble and lovely character of

Dr. Loomis without disclosing a glimpse at least of the traits that might otherwise be hidden from the knowledge of his professional associates. To his friends and family no such revelation is necessary. It may, in fact, add to their sense of loss to thus recall while their sorrow is fresh the personal characteristics which endeared Dr. Loomis to those who were permitted to share in his inner life and to enjoy his love and confidence.

To them indeed his memory will remain as a precious legacy, but to the profession he adorned and to the city he loved, the benediction of his life will be perpetuated in institutions of learning and monuments of science which will endure as long as the city of New York shall be worthy of the good works and fair fame of the noble men who in the past have sought to build its greatness upon the firm foundations of truth, justice and charity. Among these benefactors it seems to me that no name will be held in more grateful and precious remembrance than that of Dr. Alfred L. Loomis.

Stated Meeting, May 16th, 1895.

CLINICAL AND BACTERIOLOGICAL STUDY OF THE GONOCOCCUS (NEISSER) AS FOUND IN THE MALE URETHRA AND IN THE VULVO-VAGINAL TRACT OF CHILDREN.

A PAPER BASED ON EXPERIMENTS CONDUCTED IN THE BACTERIOLOGICAL LABORATORY, COLLEGE OF PHYSICIANS AND SURGEONS, COLUMBIA COLLEGE, NEW YORK.

By HENRY HEIMAN, M. D.

Believing that there is still a doubt in the minds of some medical men regarding the identity of the gonococcus in the male urethra with that of the vulvo-vaginal tract of children, and to clear myself of scepticism, at the same time studying the relative importance of the various theories of others, I planned and executed the following experiments. These studies were made upon material from *(a)*, the normal and pathological male urethra, *(b)* the normal and pathological vulvo-vaginal tract of children.

In these experiments I used the various media hitherto employed by others, and also employed a culture medium never published, viz., chest-serum agar, a medium upon which the gonococcus can be readily cultivated and to which it can be transplanted. The method of obtaining this I shall describe below. However, I must state, in fairness to Dr. Kiefer, that he availed himself of hydrothorax and ascites serum simultaneously with my experiments. He also employed the medium recommended by Finger, namely, urine agar, but without obtaining the

results which Finger claimed that he obtained. My experiments further included inoculations of a diplococcus found in the normal urethra, and of the gonococcus in man and in the lower animals. The clinical material which formed the nucleus of my experiments was obtained in part from the pediatric clinic of Dr. Sarah Welt, at the Mount Sinai Hospital Dispensary, and latterly from the genito-urinary clinic of Dr. Hayden and the pediatric clinic of Dr. Huber, both of the College of Physicians and Surgeons, and from the clinics of Drs. Goldenberg and Dessau, to all of whom I take this opportunity of expressing my deep sense of obligation.

Without wishing to burden you with the history of the gonococcus, it is sufficient to state that we are indebted to Neisser for its discovery in 1879. Since then Bokai, Bockhart, Leistikow, and others have endeavored to obtain a suitable medium for its isolation. To Bumm, however, must be given the credit of first having isolated the gonococcus, the results of his experiments having been published in his elaborate monograph. In 1892 Wertheim described a method of making plate cultures and isolating the gonococcus, which gave very good results. Finger, in June, 1894, believed that he had found urine-agar a good medium for the cultivation of the gonococcus. Turro, in July, 1894, published a preliminary paper on the cultivation of the gonococcus, namely, in an acid medium, acid gelatin and gelatin-gelose (*i. e.*, gelatin agar).

Examinations of Material from the Normal Urethra.—My experiments were conducted in the following manner: The glans was first thoroughly cleansed in soap and water; and the meatus held open while a sterilized platinum hook was carefully introduced into the urethra. In the secretions thus obtained, I found on cover-glass preparations short and long bacilli, cocci, and small and large diplococci, some of which corresponded morphologically

with the ten kinds of bacteria claimed to have been found in the secretions of the normal urethra by Lustgarten and Mannaberg. I then sowed this material, not only on gelatin and agar plates, but also on chest-serum and blood-serum agar, media which were not employed by Lustgarten and Mannaberg. I will state at the outset that I have not been able to demonstrate the gonococcus in the normal urethra. Of the micro-organisms found in the healthy urethra, the large diplococcus predominated, while none of the cocci and diplococci became decolorized by Gram's method. Of the various forms of bacteria found in the normal urethra, I have paid special attention to a large diplococcus.

(Fig. 1.) Name, diplococcus urethræ; occurrence, in male urethra. Grouping, diplococci; size, larger than the gonococcus; color, negative; temperature, room and body. Relation to air, aërobic. Gelatin plates, shape, more or less circular. Colony: Size from 1 to 3 mm.; edge sharply defined, regular; texture, granular; color, steel blue refraction, circular spot of refraction. Gelatin tubes, surface culture, slight growth, whitish liquefying; puncture, slight growth and liquefying.

(Fig. 2.) Agar plates: shape circular; size, 2 to 4 mm.; edge sharply defined, irregular; texture, granular; color, yellowish. Agar tubes, abundant surface growth; puncture, slight growth, grayish-white appearance. Glycerin agar tubes, surface growth abundant, whitish appearance. Urea agar, puncture, slight growth; chest-serum, floccular growth. Potato, grayish white growth. Gelatin-gelose, surface cultures, abundant growth; whitish. The same in puncture. Milk coagulated (reaction acid), forming clot, with small amount of serum. Fermentation broth, faintly turbid, white deposit, no gas formation. Uschinsky's medium, slight floccular growth, clear. Acid-gelatin tube, surface growth, whitish growth, the same in puncture, did not liquefy in gelatin. Wuertz's

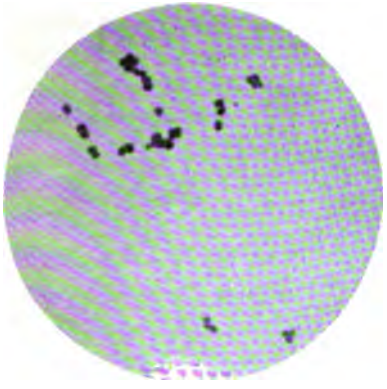

FIG. 1.—Pure Culture of Large Diplococcus from Normal Urethra, Nutrient Agar Plate.

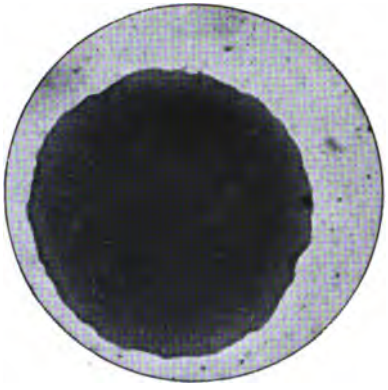

FIG. 2.—Colony of Large Diplococcus from Normal Urethra, on Nutrient Agar.

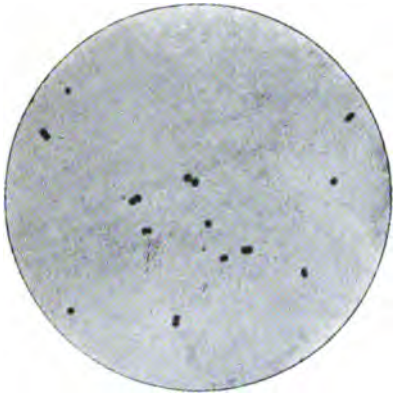

FIG. 3.—Pure Culture of Diplococcus Urethræ (Turro), on Acid Gelatine.

agar, whitish growth, turned red, acid. Pathogenesis, non-pathogenic.

Examinations of the Material from the Pathological Urethra.—The fact that Turro's acid medium was simple and easily available caused me to utilize it as my first experimental medium for the cultivation of the gonococcus. I have strictly followed his descriptions. Turro tells us that gonorrheal urine is alkaline, and upon standing becomes acid. In 16 cases examined by me it was found that gonorrheal urine, immediately after passing, gave in 15 cases an acid and in 1 a neutral reaction; 3 cases were neither treated locally nor internally; the others were treated with salicylic acid. Turro further states that by placing gonorrheal urine in an incubator for twenty-four hours he obtained a pure culture of the gonococcus, and the addition of 1 per cent. pepton increased its growth. I repeated this experiment and obtained a diplococcus which did not decolorize by Gram's method, and therefore believe it not to be the gonococcus. It is also claimed by Turro that he obtained almost a pure culture of the gonococcus in sowing gonorrhœal pus in sterilized normal urine. In repeating this experiment, I obtained also a negative result. I claim that Turro's descriptions were deficient in that he did not state the degree of acidity of his culture medium. In my experiments the degree of acidity of Turro's culture medium, gelatin-gelose, was accurately determined, and reached 0.52 acid to phenolphthaleine. The following gonorrhœal cases were examined:

Case I.—Pus from male urethra; aged thirty-five; duration eight days; no internal or local treatment. First infection cover-glass preparations show gonococci decolorized by Gram's stain. Three gelatin-gelose plates were made; each one presented cocci not decolorized by Gram's method. Involution form observed; the colony and culture appearance corresponded to the description given by Turro.

CASE II.—Pus from vulvo-vagina; aged two and a half years; three weeks' standing; acquired in hospital; cover-glass preparations show gonococci. Two plates were made. Plate I showed bacilli; Plate II, cocci, not decolorized by Gram's method.

CASE III.—Pus from male urethra; cover-glass preparation showing gonococci. Culture experiments with Turro's medium negative.

CASE IV.—Pus from male urethra; cover-glass preparation showing gonococci decolorized by Gram's method. Plate I shows gonococci not decolorized by Gram's method.

CASE V.—Pus from male urethra; cover-glass showing gonococci. Two plates. Cultures simulating in appearance Turro's description, which, however, did not decolorize by Gram's method. Plate I shows cocci not decolorized by Gram's method. Culture appearance the same.

CASE VI.—Pus from male urethra; cover-glass shows gonococci; of the two plates made both showed cocci not decolorized by Gram's method.

CASE VII.—Pus from male urethra; aged twenty-six; two weeks' standing. Has had gonorrhœa three weeks; was treated locally and internally. Cover-glass negative. Plate I shows a coccus not decolorized by Gram's method, and culture does not correspond with Turro's description.

CASE VIII.—Pus from male urethra; four weeks' standing; was treated internally; no local treatment; had gonorrhœa previously. Plates I and II show cocci not decolorized by Gram's method. Culture appearance corresponding to Turro.

Turro[1] has successfully inoculated dogs with diplococci, producing a catarrhal urethritis. His result, then, might be fairly claimed to be the first time that a catarrhal inflammation in the urethra of dogs was produced by inoculation of a micro-organism obtained from the human urethra. Turro attributes his success to an increased virulence of the bacterium by the acid medium. Up to date

[1] Lundstrom: His inoculations on dogs with a diplococcus from the male urethra resulted in an ulcerative urethritis.

the only article that has appeared by Turro on this special subject fails to mention the fact that the majority of dogs suffer from either posthitis or balanitis. It is worthy of note that the temperature of dogs fluctuates, the average temperature being 39° C. With your permission I will narrate my own inoculation experiments on dogs with Turro's diplococcus urethræ.

Dog I.—Mongrel race; male; had a slight urethral discharge; cover-glass shows pus, epithelial cells and numerous bacteria; cultures taken from urethra on acid-gelatin show cocci not decolorized by Gram's method. Inoculated the diplococcus (Turro) from gelatin-gelose (second generation). After twenty-four hours dog eats little; discharge somewhat increased. Temperature, 38.7° C. Cultures of material taken from urethra on acid-gelatin and gelatin-gelose and N. agar show cocci not decolorized by Gram's method. N. agar plates show cocci and bacilli, the former not decolorized by Gram's method. Forty-eight hours after inoculation dog eats better; discharge less; cover-glass shows pus cells and bacteria. Seventy-two hours after inoculation dog appears well; discharge is very scanty; tongue moist; appetite is good.

Dog II.—Mongrel race; male; has the usual urethral discharge; cover-glass shows pus-cells, numerous bacteria, and cocci not decolorized by Gram's method. Temperature before inoculation, 39.1° C. The material from the urethra was examined before inoculation on acid-gelatin, agar, and gelatin-gelose; cocci and numerous other bacteria were found. Twenty-four hours after inoculation with Turro's diplococcus, temperature of dog was 39.1° C.; appears to eat well; tongue moist; discharge less. Cover-glass shows numerous bacteria and few pus-cells. Forty-eight hours after inoculation, temperature 38.5° C.; other symptoms about the same. Cover-glass shows numerous bacteria. Seventy-two hours after inoculation the dog appears the same as before inoculation.

Believing that my experiments were deficient on account of the degree of acidity of my medium, I thought it advisable to repeat them. My second medium of gel-

atin-gelose and acid-gelatin responded respectively 0.40 and 0.42 acid to phenolphthaleine. I then examined ten other cases of gonorrhœa of various durations. Cover-glass preparations before sowing pus on medium showed gonococci decolorized by Gram's method. I again obtained characteristic cultures as described by Turro; a diplococcus about the size of the gonococcus which did not decolorize by Gram's method. This important differential test of the gonococcus (Neisser) from other diplococci in the urethra was not mentioned in his paper. Inoculation experiments on dogs with acid-gelatin culture, 0.42.

DOG III.—Mongrel race; male; rectal temperature, 39.1° C.; presents slight discharge from urethra. Cover-glass from material from urethra shows pus-cells and numerous bacteria before inoculation. I inoculated this dog with Turro's diplococcus. Forty-eight hours after inoculation cover-glass shows pus-cells, small cocci, and numerous bacteria. Cultures taken from urethra on acid-gelatin show bacilli; those on chest-serum agar show cocci not decolorized by Gram's method. Sixty hours after inoculation dog's condition about the same.

DOG IV.—Temperature, 38.6° C.; presents discharge. Urethra examined; cover-glass shows the same as Dog I. Cultures, before inoculation on gelatin-gelose, acid-gelatin, and chest-serum agar, show short bacilli and cocci, the latter not decolorized by Gram's method. Inoculation with the diplococcus urethræ (Turro's) gave the same negative results.

DOG V.—Pug. Temperature, 39.4° C.; slight discharge. Cover-glass preparation shows the same result as in the other dogs. Cultures before and after inoculation gave also the same result. On account of the severe secondary symptoms following the inoculations, as described by Turro, I examined the last two dogs two weeks later, and then found them in the same condition as they were before inoculation. I noticed in a paper published by Dr. Wright of Boston, a foot-note stating that his experiments after Turro's were negative. In reading

Turro's article one is impressed with the earnestness of his convictions and conclusions. Nevertheless they are open to the objection that he made no inoculations with his diplococcus in the human being.

The following is the bacterial analysis table of the diplococcus found in the acid media from specific urethritis, and corresponds partially to Turro's description.

(Fig. 3.) Name. Diplococcus culture, four days' growth.

Occurrence.—Male urethra.

Size of Diplococcus.—Correspondiug to gonococcus.

Grouping.—Diplococci.

Relation to gelatin.—Does not liquefy.

Color.—None.

Temperature.—Room and body.

Relation to Air.—Aërobic.

Gelatin Plate.—Shape circular, size 2 to 5 mm., edge sharply defined and irregular, texture coarsely granular, color yellowish; most colonies have circular spots of higher refraction.

Gelatin tubes.—Surface growth grayish-white appearance; does not liquefy. Gelatin puncture, deep growth, did not liquefy on tenth day.

Glycerin-agar tubes.—Surface growth gray, white appearance, abundant growth, more than on agar and gelatin.

Broth.—Heavy deposit, somewhat turbid.

Fermentation Broth.—Both arms turbid, faint pelicle, no gas.

Agar Tubes.—Surface growth at room temperature, grayish color.

Potato.—Gray-whitish, abundant growth. Milk, whitish deposit, consisting of diplococci; reaction acid.

Urea Agar.—Surface growth slight, growth appears gray.

Puncture.—Grayish, slight growth.

Uschinsky's Culture.—Floccular, whitish appearance.

Gelatin plates.—Culture appearance as tops of ivory balls (Turro), after a few days appearing like narrow band or tape.

Acid-gelatin Tubes.—Surface growth grayish-white appearance.

Puncture.—Deep growth, grayish white appearance.

Gelatin-gelose plates.—Corresponding to appearance as on gelatin plates, except more abundant in growth at body temperature, and less transparent. Colony, shape irregular, size three to five mm., edge sharply defined, irregular, texture coarsely granular, color yellowish, colonies have circular spots of higher refraction.

Gelatin-gelose Tubes.—Surface culture grayish-white appearance. Puncture, whitish, growth turned red.

Chest Serum.—Abundant floccular growth, turbid. Wuertz's agar, surface culture grayish-white, growth turned red, acid.

My results of the experiments described by Turro having proved unsatisfactory, and being anxious to investigate the subject more thoroughly, I took up the work of Finger, Ghon, and Schlagenhaufer. These writers claim that the best medium for the cultivation of the gonococcus is urine-agar, which consists of one part either sterile or sterilized urine, plus two parts neutral or slightly alkaline broth, containing two per cent. agar and one per cent. pepton, but no salt. According to Finger, sterile urine with agar furnishes the best medium for the gonococcus; nevertheless he does not state how he obtained sterile urine. This would be very interesting. Much of my time was devoted to this point. My first experiment consisted in sterilizing a flask, then cleansing the urethra and glans, especially the meatus, and instructing the patient to urinate directly into the sterilized flask. Placing the urine of five of these cases in flasks in the incubator for twenty-four hours numerous bacteria were found to have developed. The precau-

tion was taken that the first half of the urine should wash out the bacteria of the urethra. Even this gave a negative result. With properly prepared agar test-tubes, in order to avoid contamination, I then instructed the male subjects to pass the second half of the bladder contents directly into these test-tubes. Petri's plates containing this medium developed at body temperature after twenty-four hours bacteria, and after forty-eight hours deep colonies developed in test-tubes containing N. agar plus urine. My first results with urine-agar for sowing gonorrhœal pus from urethra are as follows:

CASE I.—Agar plus sterile urine?[1] as medium. Urine, 0.53 acid to phenolphthaleine; specific gravity, 1.023. Male, aged twenty. Gonorrhœa, five days' duration. First attack; no local or internal treatment. Cover-glass shows gonococcus decolorized by Gram's method. Plate culture shows bacilli, cocci, diplococci; the last not decolorized by Gram's method.

CASE II.—Urine agar sterilized urine. Male, aged twenty-one. Gonorrhœa, three months' duration. First attack; treatment internal and local. Cover-glass from plate culture shows cocci and diplococci not decolorized by Gram's method.

CASE III.—Culture-agar and sterilized urine. Male, aged twenty-four. Gonorrhœa, four weeks' standing. First attack; treatment internal only, no local. Plate culture, same results as above.

CASE IV.—Sterile urine. Male, aged nineteen. Gonorrhœa, two weeks' standing. First attack; treatment internal only. Cover-glass from urethra shows gonococcus decolorized by Gram's method. Plate culture showed various colonies, few corresponding to those described by Wertheim. These colonies show diplococci which decolorized by Gram's method.

CASE V.—Urine-agar plus sterile urine? Male, aged twenty-four. Gonorrhœa, three weeks' standing. Cover-glass from urethra shows gonococci decolorized by Gram's method. Plate cultures negative.

[1] The interrogation point is used to indicate the fact that the urine employed was as near sterile as could be obtained at the time.

CASE VI.—Urine sterile? Male, aged twenty-two. Two months' standing. Internal but no local treatment. Cover-glass shows gonococci decolorized by Gram's method. Plate culture shows cocci and other bacteria and diplococci, but no gonococci.

CARE VII.—Agar plus sterile urine? Male, aged twenty-five. Gonorrhœa, three weeks' standing; internal treatment only. Cover-glass from urethra shows gonococci decolorized by Gram's method. Plate cultures, numerous colonies, few characteristic gonococci colonies, which decolorized by Gram's method.

CASE VIII.—Agar sterile urine? Male, aged twenty-two. Two weeks' standing; second attack; internal treatment only. Cover-glass from urethra shows gonococci decolorized by Gram's method. Plate culture negative.

The first series of experiments with urine-agar were rather discouraging to me, as I believed that the sterile urine would probably have enhanced its virtues as a medium. In this connection I must say that Dr. Alexander Lambert of this city kindly suggested and prepared for me Kitasato's filter as modified by him. Such a filter was attached to a Sprengel air-pump. In this manner I obtained sterile urine. For control experiment the urine was kept at body and at room temperature, but no development of bacteria followed. The urine before filtration reacted acid, specific gravity 1.021, and despite filtration its specific gravity and acidity remained unchanged, a point worthy of notice. Having then obtained sterile urine I had hopes of securing a perfect urine-agar medium, but my anticipations were not realized, inasmuch as the sterile urine did not cause gonococci to grow more abundantly, nor could I demonstrate any better growth of gonococci on plate or test-tube cultures. It is needless for me to state that I was careful to select pus as far back from the meatus as possible, in order to avoid the probability of mixed bacteria. Encouraged, on one hand, to find the gonococci on urine-agar with Petri's

dishes, I was discouraged, on the other hand, that I could not employ, as Finger also admits in his article, the urine-agar as a transplantation medium. It is a deplorable fact that while we have thus an available medium for the cultivation, we have not in urine-agar a perfect transplantation medium. The property of immediate transplantation I regard as an important adjuvant in the study of the biology of the gonococcus. In this connection the medium of Bumm, also Wertheim's placenta-serum agar medium, have proven of inestimable value to me.

Placenta Blood-serum.—I strictly followed the directions of Wertheim in the preparation of placenta blood-serum, but not in the application of it. For the purpose of obtaining placenta blood, I availed myself, through the courtesy of Dr. Tucker, of the facilities provided by the Sloane Maternity, New York. For convenience I have had the placenta blood collected in sterilized potato-culture test-tubes about one half full. I could then easily separate after an hour or more with a sterilized thin glass rod the coagulum within the large test-tubes. After twenty-four hours' standing in the refrigerator I drew off with a sterilized pipette the expressed serum into a smaller-sized sterilized test-tube. This was allowed to stand another twenty-four hours in the refrigerator, so that the remaining red corpuscles could settle to the bottom; the straw-colored serum was then decanted into another sterilized test-tube and placed in a liquid blood-serum sterilizer, for the purpose of sterilizing the liquid blood-serum by fractional sterilization at a temperature of 65° to 68° C. for one hour on each of six consecutive days. To control the sterilized serum tubes they were placed in the incubator for forty-eight hours. The tubes containing two parts of agar were liquified, then allowed to cool off, and placed in a water bath at 40° C., together with tubes containing serum one part. They were then poured out in Petri's Plates, and then into sterilized test-tubes as the transplantation me-

dium. Incidently I would state that in many instances pus was kept in sterilized test-tubes and taken to the laboratory five or six hours after withdrawing it from the urethra, then it was sown on plates, and in nearly every case, twenty in all, the gonococcus (Neisser) was obtained and isolated. In sowing the pus on surface cultures I obtained a similar result, especially in fresh cases of gonorrhœa, just as Wertheim did when he diluted the secretions in the serum tubes, and then mixed them with agar and poured out the pus-inoculated medium on plates. In chronic gonorrhœa I would prefer the method described by Wertheim. Thus if gonococci are present, no matter in how small a number, we are almost certain to find some single deep or surface colonies of the gonococci. After twenty-four hours I have seen colonies of the gonococci as described by Wertheim, and was always able to transplant them into chest-serum agar test-tube cultures. That I was dealing with the gonococcus (Neisser) I will show later on, in describing my inoculation experiments.

The Gonococcus Analysis Table is as follows:

Name, Gonococcus (Neisser).—(Fig. 4.)

Occurrence.—Urethra, vaginal tract.

Form.—Diplococci.

Grouping.—Mostly Diplococci.

Size, 1-4 m.—Stains with aniline dyes, best with alcoholic 2 per cent. methyl violet solution. Gram's stain becomes decolorized.

Relation to air.—Aërobic.

Temperature.—At body temperature only.

Action on Gelatin.—No growth.

Milk.—No growth.

Smith-tube.—Gas formation, no growth.

Gelatin Puncture.—No growth. Surface colonies, no growth. Deep colonies, no growth.

Gelatin Plate.—No growth.

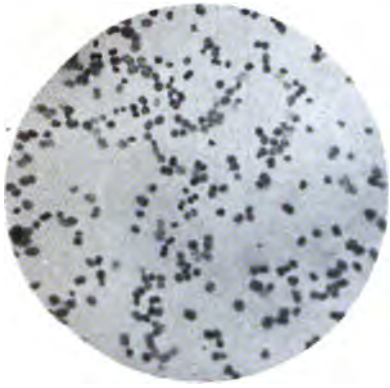

FIG. 4.—Pure Culture of Gonococcus from Male Urethra, on Chest-serum Agar.

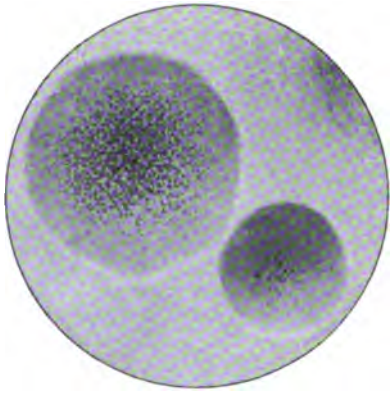

FIG. 5.—Colony of Gonococcus from Male Urethra, on Chest-serum Agar.

FIG. 6.—Pure Culture of a Diplococcus found in Colpitis Catarrhalis, on Nutrient Agar.

Agar Plate.—No growth. Agar tubes, no growth, puncture or surface.

Glycerin-agar Plates. No growth. Tubes, puncture or surface, no growth.

Urea-agar.—No growth.

Uschinsky's Medium.—No growth.

Loeffler's Coagulated Blood-serum Tubes.—No growth.

Wuertz Agar.—No growth.

Wuertz Agar+Placenta-serum.—Whitish gray growth. Medium turned faintly red.

Rosolic Acid Broth.—No growth.

Meat Broth.—No growth.

Potato—No growth.

Acid-gelatin Plate.—No growth.

Tube Puncture Surface.—No growth.

Gelatin-gelose Plates.—No growth.

G. G. Tubes.—No growth.

Urine-Agar Plate.—Slight growth, corresponding to description which I purpose to give on blood-serum agar plates.

Urine-agar+Sterile Urine.—Slight growth.

Urine-agar+Urine Filtered.—Slight growth.

Urine-agar Test tubes.—Surface slight growth. Puncture no growth.

Acid-gelatin+Placenta Serum.—No growth.

Gelatin-gelose+Placenta Serum.—No growth.

Glycerin-agar+Placenta Serum.—Slight growth on surface.

Two per cent. Agar+Sheep-serum.—Plates, slight growth.

Tubes.—Surface slight growth.

Two per cent. Agar minus Salt+Sterilized Placenta Serum. Slight growth. Tube, surface slight growth.

Two per cent. Agar minus Salt+Chest-serum.—Plates, abundant growth. (Fig. 5.)

Colonies, shape circular. Edge somewhat irregular.

Shading off into yellow-white. Texture finely granular in periphery. Color yellowish. Presents punctured spots of higher refraction in and around centre, chest-serum agar tube more abundant than on placenta, sheep-serum, or ascites fluid.

Appearance, grayish-white. Puncture growth, grayish-white appearance.

Two per cent. Agar minus Salt+Filtered Chest-serum Plates. Considerable growth. Tubes, surface growth, slight growth grayish-white appearance, serum presenting small, crystal-like bodies.

Pathogenesis, pathogenic.

Morphology.—Gonococci are found mostly in pus-cells (Fig. 8) and at times in other portions of the field, a fact probably due to the mechanical effect of smearing the pus on cover-glass or slide. They appear generally in pus-cells, as diplococci, coffee-bean in shape, or in groups of two or four, generally extra-nuclear. In pure cultures they appear as diplococci. (Fig. 9.) At times they appear as round diplococci of various sizes, especially in older cultures. Others again are irregular in shape or granular in appearance. In this connection it must be remembered that the gonococcus possesses the peculiarity termed in bacteriology "involution form" in older cultures.

In my cultures of chest-serum agar, at the expiration of about twenty-one days, this so-called "involution form" was very apparent, so much so that the micro-organism was barely recognizable as the gonococcus; nevertheless in transplanting this culture I obtained a gonococcus growth on chest-serum agar. A culture sixty-two days old, and a second, one hundred and two days old, showed in cover-glass preparation nothing resembling a gonococcus. All that could be seen was a granular mass which by Gram's method became decolorized. In view of the diversified appearance of the gonococci in older cultures under these varying conditions, opinions vary as to the

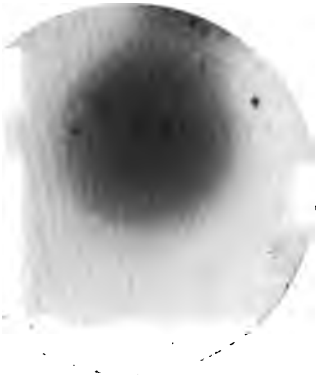

FIG. 7.—Colony of a Diplococcus found in Colpitis Catarrhalis, on Nutrient Agar Plate.

FIG. 8.—Pus from Colpitis Specifici, showing Gonococcus.

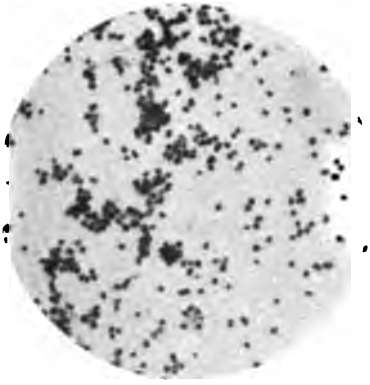

FIG. 9.—Pure Cultures of Gonococcus from Colpitis Specifica, on Chest-serum agar.

longevity and virulence of the gonococcus in chronic forms of disease. From a medico-legal and social standpoint the practical bearing of this matter is very evident. Thus candidates for marriage who are victims of a chronic urethritis and who seek advice in regard to the advisability of their contemplated marriage, can only receive an affirmative opinion, providing a thorough clinical and bacteriological examination of the secretions is executed in the following manner:

Cover-glass preparation on three consecutive days must be examined with a microscope provided with a mechanical stage and by Gram's method. Should these specimens prove negative, to exclude any possibility of doubt, cultures should be made on chest-serum agar on three consecutive days, and for such an important examination I would not recommend a surface culture, but a series of diluted chest-serum test-tube inoculations as described by Wertheim.[1]

The Gonococcus as Found in the Human Economy.—Up to date a perusal of the literature of the subject brings to light the fact that gonococci are reputed to have been found in the following regions and in about the following order of frequency: Male urethra, vulvo-vaginal tract, vesical bladder, eye, peri-urethral abscesses, glands of Bartholini, joint effusions, kidney, Fallopian tubes, peritoneum, rectum, and mouth.

The Staining of the Gonococcus by Aniline Dyes.—Of the various dyes used—Bismarck brown, safranin, methylene blue, fuchsin, gentian-violet and malachite-green—I found that fuchsin and gentian-violet are faulty in that they stain too intensely. Success with the use of these dyes depends, no doubt, upon the skill with which a thin film is smeared on the glass. As the best method for ordinary staining I would recommend a 2 per cent.

[1] It would be advisable to produce an artificial urethritis with a nitrate of silver solution for the purpose of bringing gonococci to the surface.

alcoholic methyl-violet solution. This is practically a differential stain, and consumes but thirty seconds in its execution. As a practical double stain, I employed Schütz's and Lanz's methods.

Having thus passed in review the various methods commonly employed for the detection of the gonococcus and for its differentiation stain, I am forced to the conclusion that any procedure which does not include Gram's method is imperfect and unreliable. His method is the only reliable one for the differentiation of the gonococcus from all other diplococci found in the healthy urethra and vulvo-vaginal tract. So careful an observer as Friedlander goes further and states that it is the ideal method of staining bacteria. In looking over the literature of Gram's stain, I find that a great many writers obtained different results with it. This, to my mind, is not surprising on account of finding in our text-books on bacteriology descriptions which differ materially from each other. Those of you who are interested in this particular theme can readily become convinced of the correctness of this statement by looking up any of the following authors: Crookshank, Flügge, Fraenkel, Günther, Heim, Sternberg, Hueppe, Von Jacksch, O. Israel, etc. E. C. Wendt, New York, credits Roux with having first found that the gonococcus becomes decolorized by Gram's method. Gram, in 1884, gave us an excellent description of his method. This stain was accidentally discovered by him while engaged with aniline gentian-violet sections that were placed in iodine solutions. This led him to study the subject more in detail, with the result that he gave us an excellent method for staining bacteria in sections and cover-glass preparations. In the preparation of the aniline gentian-violet solution Gram refers us to Friedlander's book, and out of the many solutions found therein I have selected the Koch, Ehrlich, aniline-water, gentian-violet solution, as the best for illustration. I have prepared

it as follows: To 10 c. c. of water add 2 c. c. of aniline oil, shake well and filter through moist filter paper. To the clear aniline water obtained add 1 c. c. of 97 per cent. alcohol and 1 c. c. of a concentrated alcoholic gentian-violet solution. I have given these proportions, as this is about the quantity required for ordinary staining. After passing the cover-glass quickly through the flame, it is then placed in this solution from two to three minutes. Gram next advises that the cover-glass may be washed in alcohol or not. I preferred to drain off the excess on filter-paper without washing in alcohol. The cover-glass is then placed for five minutes in Gram's iodine solution, which consists of iodine, one part; iodide of potash, two parts; water 300 parts, and then placed directly into alcohol 97 per cent., in order to wash out all the coloring matter. It is sometimes advisable to renew the alcohol. Now wash the cover-glass well in water and place it in a Bismarck-brown solution, which consists of a concentrated aqueous Bismarck-brown solution, one part; water, five parts, for one or two minutes, then wash in water and mount in balsam. For differential stain, instead of Bismarck-brown I have employed fuchsin and safranin, only to find that the Bismarck-brown gave the best results. It is to be regretted that we have not at present a specific stain for the gonococcus, as we have for the tubercle bacillus.

Description of the Various Culture Media Employed for the Gonococcus.—In 1885 Bumm first successfully cultivated the gonococcus, using a mixture of beef and human blood-serum. Then Wertheim employed the placenta blood-serum described by me above. My charts have shown that of all media used for cultivation, the chest-serum agar gives the most abundant growth of the gonococcus. Various other fluids of the human body have been employed by different investigators with varying results—peritoneum serum, chronic synovitis effusions,

hydrocele and pericardial fluids, the fluid contents of ovarian cysts, and hydro-salpinx fluid. I have employed placenta serum, sheep blood-serum, peritoneum serum, filtered chest-serum, and found that liquid sterilized chest-serum plus two per cent. agar broth plus one per cent. peptone with or without one-half per cent. salt, gave me the best results. This fact I could attribute only to the great amount of serum albumen found in chest-serum. The value of this serum can be demonstrated by placing a flask containing chest-serum in a refrigerator, allowing it to stand over night, when we will find a clot has been formed and that the supernatant serum appears to be of a more tenacious consistency than placenta, peritoneum, and beef serum, when treated in the same way. I strictly controlled my experiments with plate and test-tube cultures by mixing sterile beef-serum with two per cent. agar; sterile peritoneum serum plus two per cent. agar; beef-serum plus two per cent. agar; placenta serum plus two per cent. agar; chest-serum plus two per cent. agar; and found that all of these chest-serum plus two per cent. agar, etc., showed the most abundant growth of the gonococcus in plate and in test-tube cultures. One of the plates will show almost a pure gonococcus culture obtained from urethral pus. (Fig. 10.)

The chest-serum was obtained in one case from a patient suffering from hydrothorax, with a temperature of 38.3° C. In another case, from a patient suffering from an acute pleuritis, with a temperature of 39.5° C. From each case one litre was withdrawn.[1] I rarely found it necessary to pour the supernatant chest-serum into another chest-tube, as is done in placenta serum, owing to the number of red blood-corpuscles present in the latter serum. Not being satisfied with these results and desiring to avoid the long and tedious fractional sterilization of liquid

[1]To Dr. Ewing I am indebted for supplying me with chest and peritoneum serum.

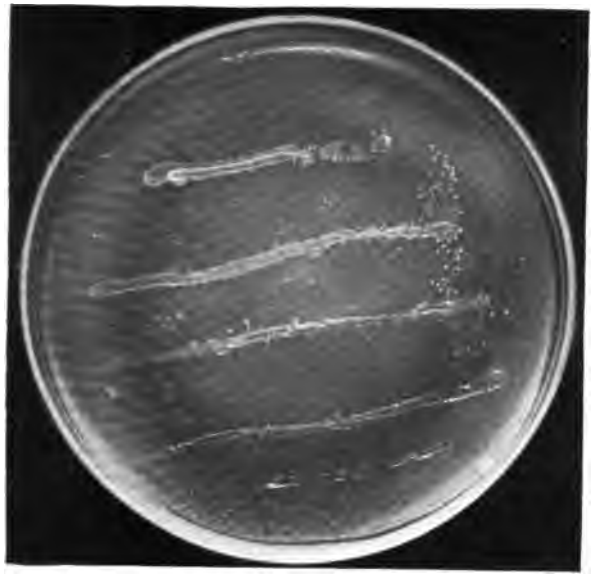

FIG. 10.—Gonorrhœal Pus sown on Chest-serum Agar, showing a Pure Gonococcus Culture.

FIG. 11.—Diplococci found in Pus-cells of Catarrhal Colpitis, not decolorized by Gram's Method.

serum, I was anxious to obtain a method of obtaining sterile serum, as I obtained sterile urine. I therefore resorted to the filtration method. I found chest-serum on account of its tenacity difficult of filtration. At the rate of six hours per day for one week, using Reichel's filter with the aid of Sprengel's air-pump, only 200 gm. of filtered chest-serum were obtained. In order to render the chest-serum more liquid, I first filtered it through sterilized absorbent cotton, placed in a sterilized glass funnel, but this gave about the same result. The only advantage derived from the filtered sterile serum was its extremely clear appearance. For the cultivation of the gonococcus, however, in plate and in test-tubes, it gave a less abundant growth than the sterilized chest-serum and gave a result about equal to that of placenta-serum agar.

In my efforts to procure sterile serum I followed the suggestion of Dr. Prudden, namely, to add to the chest and peritoneum kept in sealed flasks and test-tubes one-half per cent., or one per cent. of chloroform. Before adding chloroform, control experiments were made in the following way: a half-dozen test-tubes containing peritoneum serum were left uncorked and exposed to the air for forty-eight hours; a second half-dozen containing peritoneum serum were at once sealed after one per cent. or one-half per cent. chloroform was added. At the end of forty-eight hours chloroform was added to the uncorked tubes, which were then sealed. Thirty days later a great number of bacteria were nevertheless found in the half-dozen test tubes that were at first left exposed to the air. Among the second half-dozen test-tubes were a few containing no bacteria. These were treated with agar, peptone, and salt, and could be used as a culture medium, notwithstanding the addition of the chloroform. I regret to say that I could not complete these experiments for this publication.

Studies in the Normal Vulvo-vaginal Tract in Children.—In looking over the literature on this subject, I

found that little has been written. The most frequent allusion to this topic is in connection with the vulvo-vaginal tract during pregnancy. Döderlein examined the lochial discharges and found that, where the lochia were profuse, staphylococci and streptococci were abundant. Krönig also found streptococci and staphylococci, and believed that streptococci become non-virulent owing to the hyperacidity of the vaginal secretions. G. Thomen found in three out of seven cases of lochial discharge streptococci, although none could be demonstrated in the vagina and cervix. My investigations in connection with the normal vulva were principally made for the same purpose as those in the normal male urethra—that is, I endeavored to ascertain various kinds of bacteria which cover-glass preparations and ordinary culture media bring to light. I also studied the vulvo-vaginal secretions and urethra of children, employing media upon which the gonococcus thrives. Out of twenty cases examined in this manner, in no single instance have I been able to demonstrate the gonococcus (Neisser). Cover-glass preparations showed ephithelial cells, and in some cases quite a number of pus-cells, numerous bacteria, cocci, diplococci, short bacilli, large pear-shaped bacilli with possible spore formation (?), smegma bacilli, and spirilla. The ages of the children examined varied between three months and thirteen years. The secretions were taken from all parts of the vulva, without cleansing the parts at all. Whenever urethral discharge was taken the vulva was cleansed with water. I have met a class of cases with very slight mucopurulent discharge which the parents were not aware of. Then again there were other cases whose only symptom was enuresis. This latter class of children attracted my attention especially, as they turned out to be simply a catarrhal colpitis, clinically and microscopically. Cover-glass preparations showed a great number of pus-cells, containing diplococci, partly resembling in size and form

the gonococcus (Neisser), also small cocci and numerous other bacteria (Fig. 11). These diplococci were found independently of the methods of Bockhart, E. Fränkel, Koplik, and others. This fact led me to believe that if there was such a micro-organism as a pseudo-gonococcus, the diplococci of the vulva, found in the pus-cells and partly resembling the gonococcus, these certainly would deserve the denomination.

I believe that the term pseudo-gonococcus, as at present applied, is apt to mislead. It would be wiser to term the micro-organism found in the pus-cell of the catarrhal form of colpitis, diplococcus colpitis catarrhalis. Whether this micro-organism is pathogenic and infectious, I am not prepared to state at present. After I had examined the discharges of my catarrhal colpitis cases, which clinically did not appear as gonorrhœal cases, and obtained negative results, I came to the conclusion that these cases of colpitis must be attributable either to the diplococcus found and isolated by Bockhart or to causes as yet unknown. The best proof of the correctness of this assertion is the fact that these diplococci found in the pus-cells, which partly resemble the gonococcus, will not decolorize by Gram's method, as my plates and slides will show. Bockhart, in 1886, gave a thorough description of a small diplococcus found in the vagina and proved to be the origin of non-specific urethritis. He was able to secure a fair growth of these small diplococci on agar plates and a more abundant growth on blood-serum at body temperature. His inoculation experiments with the small diplococcus in the human urethra gave a positive result. He produced an artificial urethritis, the secretions of which showed the small diplococci in pus-cells. E. Fränkel describes cases of endemic infectious colpitis which I believe to be analogous to Bockhart's. E. Fränkel inoculated the eyes of rabbits with secretions from catarrhal colpitis cases, with negative results. His inoculations on the human eye gave a positive

result. In one out of three inoculations he observed on the fifth day a catarrhal condition of the eye, the secretion from which gave the same diplococci as those in the secretions of the vagina. He does not mention the Gram stain at all for cover-glass preparations, except as applied to sections, and even then he found no bacteria. In a later announcement he describes experiments in trying to isolate this diplococcus. He obtained several forms of diplococci described by Bumm, and a diplococcus not described by Bumm, with which he inoculated the human eye with negative results. On one occasion he produced in his inoculation experiments a purulent conjunctiviti in a rabbit, which lasted five days, but could not find the characteristic diplococcus on account of the prevalence, as he claimed, of other bacteria. This experiment of E. Fränkel would appear fallacious, owing to the fact that secretions and not pure cultures were employed. E. Fränkel considers his diplococcus to be the real gonococcus, only modified in its behavior from its successive growths. My attention was especially directed to a diplococcus found in the catarrhal form of colpitis. This micro-organism simulated in appearance the one found in the pus-cell of the secretions of catarrhal colpitis. I studied the biology of this diplococcus, as my table will show (Fig. 6).

DIPLOCOCCUS.

Occurrence.—In catarrhal form of colpitis of children.
Form.—Diplococci and cocci.
Grouping.—Diplococci and cocci.
Size.—About size gonococcus, 1 to 4 micro-millimetres.
Color.—None.
Temperature.—Room and body.
Relation to Air.—Aërobic.
Gelatin Plates.—Colony.
Shape.—Circular and elliptic.
Size.—From 3 to 5 micro-millimetres.

Edge.—Sharply defined.

Texture.—Coarsely granular.

Color.—Yellowish brown refraction.

Gelatin Tubes.—Slight surface growth, whitish appearance. Did not liquefy. The same in puncture. Acid-gelatin plate. Texture finely granular, otherwise the same as in nutrient gelatin plate. Acid-gelatin surface, slight whitish growth not liquefying; the same in puncture.

Nutrient agar plates.

Colony.—Shape circular. (Fig. 7.)

Size.—Three to 5 mm.

Edge.—Sharply defined, regular.

Texture.—Granular.

Color.—Yellowish.

Nutrient Agar Tubes.—Abundant growth, pale yellow.

Glycerin Agar Tubes.—Slight surface growth, whitish-gray.

Urea Agar.—Surface culture and puncture, no growth.

Chest-serum.—Floccular deposits; medium clear.

Potato.—Yellowish growth; slightly spreading.

Milk.—Some white sediment; reaction acid; not coagulated.

Fermentation Broth.—Growth in both arms, but cloudy in long arm; white deposit; slight pellicle.

Uschinsky's Medium.—Slight growth; clear.

Wuertz's Agar.—Abundant growth on surface medium; becomes blue above; lower part unchanged; alkaline; the same in puncture.

Pathogenesis.—Negative in one case in the normal male urethra.

Vulvo-vaginitis of Children.—Vulvo-vaginitis, urogenital blenorrhœa, or colpitis gonorrhoica in its various forms must have been just as prevalent years ago as it is to-day, only that its nature and causation were less understood. Little was mentioned of this condition in the medical works of the day. Thus in such old works as that of Adolph

Hencke, 1821, and that of C. M. Billiard, 1850, no mention whatever is made of vulvo-vaginitis. Mr. Bouchut in 1855 does not mention anything of vulvo-vaginitis. Dr. W. H. Day in 1881 describes discharges of a muco-purulent character during dentition from intestinal irritations, ascarides in the rectum after scarlet fever and other fevers, but does not mention its identity with gonorrhœa. Vogel in 1887 describes catarrhal and gonorrhœal forms of vulvo-vaginitis. Ashby and Wright, 1889, speak of vulvo-vaginitis, and state it is due to neglect, dirt, irritation by thread-worms, and at times the result of contact with discharges from vulvitis. J. Lewis Smith in 1890 speaks of a catarrhal and gonorrhœal variety. In Keating's Encyclopœdia, 1890, vulvo-vaginitis is considered in two varieties: First, a simple catarrh due to dirt, irritation, etc., and second, the infectious varieties due to the gonococcus. Henoch in 1892 described an infectious form, but states that he attaches little importance to such gonorrhœal secretions for the reason that gonococci can be demonstrated in cases of vulvo-vaginitis, in which no history of gonorrhœa can be proven. Baginsky in 1892 describes a vulvo-vaginitis due to mechanical irritation or to worms; second, vulvo-vaginitis catarrhalis et blenorrhoica, caused by the same factors, and also mentions an endemic form of vulvo-vaginitis due to an organism similar to the gonococcus, or depending upon the gonococcus. I can endorse his opinion. I have found the true gonococcus and another diplococcus in pus-cells in one and the same specimen, the true gonococcus showing the decolorization by Gram's method, the other diplococcus in the pus-cells partly resembling the gonococcus, retaining the aniline gentian-violet stain. Baginsky also describes other varieties of vulvo-vaginitis. Star, 1894, divided vulvo-vaginitis into a catarrhal, or irritative, and a gonorrhœal form.

On reviewing the literature on the clinical and bacterio-

logical studies of vulvo-vaginitis, I find as early as 1878, in an article written by J. Morris, that mention is made of the fact that girls have been affected through the agency of clothes, and of the difficulty of differentiating the specific from the non-specific variety. Neisser states that fluor vaginæ is due to the gonococcus, and that ordinary vaginal secretions contain all forms of bacteria, except gonococci. Pott regards purulent vaginitis to be of specific origin, and gives other causes. Cseri found the two varieties of diplococci in pus, and claims that the one variety is identical with E. Fränkel's diplococci. He cites, however, no culture experiments. Dusch reports an epidemic of vulvo-vaginitis in hospital practice, and speaks of the possible infection by the use of the same thermometers, chambers, toys, etc. Hofmokl speaks of a spontaneous occurrence of vulvo-vaginitis, and as special etiological factors cites malnutrition, scrofulous, tuberculous, rachitic, and specific affections, scarlet fever, measles, scabies, masturbation, oxyuris-vermicularis, and states that he has observed the same trouble in apparently healthy children. He rarely observed the gonorrhœal type, and gives the age of occurrence between eight and twelve years. No microscopical examinations were made by him. Spaeth reports vulvo-vaginitis cases to be of gonorrhœal origin. No cultures were made by him. R. Skutsch cites the case of two hundred and thirty-six school-girls who were attacked with vulvo-vaginitis after bathing. Some of these girls were afterward affected with ophthalmia blenorrhoica.

Epstein gives no etiological data for catarrhal vulvitis except uncleanliness, and uses the term "desquamative catarrh." Cassel gives accurate clinical and microscopical descriptions of cases, but he does not state that he employed culture media. Martin claims that the differentiation between gonococci and other diplococci found in pus-cells must be still considered impossible. In refu-

tation, so far as my experiments go, they seem to prove that this matter of differentiation of the various diplococci is not *sub judice*, but a certainty by the aid of Gram's method. The inoculation experiments of Martin in the male urethra are open to the serious objection that he did not avail himself of a pure culture, but simply of the secretions. Koplik describes a diplococcus found in the pus-cells of the secretions of diseased vulvo-vaginæ of children. He enumerates three varieties of vulvo-vaginitis, viz.: that due to uncleanliness, manifesting itself by painful micturition and a slight serous discharge; second, a catarrhal type with slight purulent discharge; and third, a true gonorrhœa. E. Berggrün examined bacteriologically thirty-one cases of vulvo-vaginitis, eleven of which gave positive results on cover-glass preparations and on blood-serum agar, Berggrün himself admitting, in the case of catarrhal type of vulvo-vaginitis, his inability to ascribe a reason for this. He divides his purulent cases first into a specific type; second, a type due to trauma; and third, a catarrhal form which he believes to be infectious.

Experiments on the Vulvo-vaginal Tract of Children.—It is a notorious fact that in many instances the etiology of vulvo-vaginitis of children is shrouded in an atmosphere of obscurity. There is, however, another class of cases in which the etiological factor can be readily ascertained to be a direct infection from adult to child. The endemic variety originates at times in hospitals. Many cases that came under my observation at my clinic in the Mount Sinai Hospital Dispensary gave such a history of origin. These patients, it was also ascertained, presented on closer inquiry no symptoms referable to the vulvo-vaginal tract, on their admission into the hospital, where the infection took place. An important truism derived from the knowledge is that in isolation we have a preventive for the spread of vulvo-vaginitis. I believe the law which makes it compulsory to isolate such infectious diseases as diph-

theria, scarlet fever, etc., would be just as applicable to cases of vulvo-vaginitis. The consensus of opinion seems to be unanimous on this point, that stuprum or direct contact, bedding, bath-tubs, thermometers, clothing, toys, chambers, closets, instruments, etc., are carriers of the poison of the disease. It is to be regretted that up to date no perfect classification of the various types of vulvo-vaginitis is extant. The classifications hitherto offered for acceptance, are, as a rule, mixed types. I take the liberty of offering the following: First, a catarrhal form, subdivided (*a*)into those due to uncleanliness, foreign bodies, and trauma; (*b*) infectious, but not gonorrhœal. Second, (*a*) a gonorrhœal type, due to gonococcus (Neisser); (*b*) the mixed infection due to gonorrhœa and possibly other diplococci. I shall not utilize as statistical material results of my former examinations of the discharges from the vulvo-vagina, for the reason that I did not employ the Gram stain. In my last set of cases I always confirmed the presence of the gonococcus by Gram's method, not only in cover-glass preparation, but also in culture media. *En passant*, I shall give the history of a few striking and convincing cases.

Case I.—Aged twenty-two months, presented a purulent vaginal discharge of fifteen days' standing. Was treated locally. Father of the child had gonorrhœa of three weeks' standing, with profuse discharge, and stated that he and the child occupied the same bed. Cover-glass preparation from secretion of father and child showed gonococci decolorized by Gram's method. Chest-serum agar plates employed in the case of father and child revealed gonococci, which I could readily isolate.

Case II.—Aged two and a half; five days' duration; no treatment. Mother and father both had discharges. Cover-glass of mother and child revealed gonococci decolorized by Gram's method.

These two cases prove almost conclusively that the gonococcus is the etiological factor.

CASE III has been of special interest to me on account of the long duration of the discharge. The child was three years old, and presented uro-genital blenorrhœa of ten months' standing. At times there was an acute exacerbation. The cover-glass shows two kinds of diplococci: First, the gonococcus which decolorized by Gram's method; and second, diplococci in pus-cells which appear somewhat larger than the gonococci, but were not decolorized by Gram's method.

In the latter class of cases, the question would naturally arise whether we were not dealing with a mixed infection. The biology and pathogenesis of the gonococcus of the vulvo-vagina is identical with the gonococcus of the male urethra, as my culture and inoculation experiments will show. The fact that vulvo-vaginitis is so little amenable to treatment by ordinary methods is partly proof that we are dealing with the same infection as in male gonorrhœa.

Inoculation Experiments.—The first thought that occurs to our minds is whether absolute reliability can be placed in the results obtained from cultivating the gonococcus on such media as agar, potato, gelatin, nutrient gelatin, and acid-gelatin. In the first place, the conditions under which the experiments were conducted by different workers were not always the same, the personal equation always entering as an important factor. Thus various investigators may view the same microscopic field, and yet the interpretations of their conceptions may be as numerous as the investigators themselves. In the case of the gonococcus this rule applies with double weight, since there is a deplorable scarcity of media available for the cultivation of this diplococcus. There are, however, other specific bacteria to which this rule does not apply at all. Thus through the efforts of bacteriologists we are in a position to-day to differentiate a number of organisms through their characteristic growth on special media. The *bacterum coli commune*, for instance, is an acid-producer,

while the typhoid bacillus is not. The crucial test for the establishment of the gonococcus on a true bacteriological basis is inoculation experiments in the human economy, since animals are to a certain extent refractory. And yet how seldom this method has been brought into play in connection with works of others becomes apparent when we look up the literature of this special topic. For we then learn that such careful investigators as Bockhart, Oppenheimer, and Kreis, did not make inoculation experiments. Their results are therefore considered questionable by Finger and Wertheim. Sternberg in 1884 thought he was dealing with the gonococcus, and yet when he came to his inoculation experiments found he was not dealing with the gonococcus at all. Recognizing the importance of inoculation experiments, my attention and experiments were principally directed to that field in order to establish the true pathological significance of the gonococcus Neisser.

It becomes my pleasant duty here to acknowledge my sincere appreciation and a sense of deep obligation to Dr. Ira Van Gieson for his painstaking efforts to further my researches by obtaining material for my inoculation experiments. He was kind enough to control my first inoculation experiments and was of service to me in many other ways. Having obtained a supposed pure culture of the gonococcus, I utilized a fifth generation on chest-serum agar. The culture in question was made from pus obtained from the male urethra.

Case I.—The patient selected, a boy aged four, suffering from idiocy, complicating chronic epilepsy. He was badly nourished. Pulse, 84; respiration, 18; temperature, 37.1° C. Having cleansed his glans penis, a sterilized platinum hook was passed into the urethra to obtain secretion for the cover-glass preparation. The specimen showed epithelial cells containing bacteria, none of which decolorized by Gram's method. To further control the urethra, plate-cultures on nutrient gelatin, nutrient agar,

and chest-serum agar were made. The usual forms of urethral bacteria were obtained, none of them, however, decolorized by Gram's method. The urethra was then inoculated with a supposed pure culture of the gonococcus. Nothing was noticed up to the fifth day. In the presence of Drs. J. J. Collie and J. R. Knapp I was able to express from the urethra a drop of pus. Cover-glass preparations were made which showed pus-cells containing typical gonococci decolorized by Gram's method. To complete the experiments plate-cultures were made on chest-serum agar, nutrient agar, and gelatin. I was agreeably surprised to obtain on chest-serum agar the same growth which previously was considered to be the gonococcus (Neisser), and was also able to isolate it in pure culture. The subsequent course of the disease was a mild one, the temperature (rectal) never rising above 37.7° C. On the twelfth day a very appreciable quantity of pus could be expressed. Cover-glass preparation showed quite a number of typical gonococci in pus-cells, decolorized by Gram's method. On the nineteenth day the same condition was found. On the twenty-fifth day the quantity of pus was profuse. Temperature, 37.4° C.; pulse, 120; respiration, 30. Cover-glass preparation showed numerous diplococci decolorized by Gram's method. Having thus convinced myself of the specificity of the gonococcus of the male urethra, I was tempted to repeat the experiment with the supposed gonococcus obtained from vulvo-vaginitis of children. For this purpose I therefore selected a fourth generation. The case selected was an idiotic boy, aged sixteen, who had been under observation for about four years. No history of ever having had a discharge from the urethra; otherwise healthy. On the day of inoculation the temperature, 38.5° C.; respiration, 28; pulse, 120. The same precautions were observed as in the other case. Cover-glass preparation showed epithelial cells, no bacteria. Plate-cultures were taken on gelatin, agar, and chest-serum agar. The gelatin plate gave negative results, the agar plates developed a coccus not decolorized by Gram's method, and the chest-serum agar-plate showed cocci and streptococci in great abundance, but not decolorized by Gram's method. With a sterilized platinum hook a pure culture of the gonococcus of vulvo-vaginitis from chest-serum agar was inoculated; the precaution was taken to have the patient empty

his bladder before the inoculation. On the second day pus appeared. On the fourth day the patient presented a swollen penis, with a profuse discharge. Temperature, 38° C.; respiration, 21; pulse, 126. The temperature in this case was reduced 1° C., but the pulse increased from 120 on the day of inoculation to 126. Cover-glass preparation showed typical gonococcus decolorized by Gram's method, and from the urethral discharge I was again able to cultivate and isolate the gonococcus (Neisser).

Case II.—Adult, aged twenty-six, in the last stage of tuberculosis. Has been under observation for two years; denied any gonorrhœal history. Temperature, 40.2°C.; pulse, 150; respiration, 36. The hyperpyrexia in this case made it of special interest, as Finger claims that inoculation experiments showed only in subjects with nearly normal temperatures. Cover-glass preparation of Case II., as well as gelatin, agar, and chest-serum agar plates, showed numerous bacteria not decolorized by Gram's method. A pure culture of the gonococcus obtained from vulvo-vaginitis was inoculated into the urethra. On the second day no discharge was present, and patient did not complain of painful micturition. Temperature, respiration, and pulse about the same. On the fourth day a slight discharge was noticed. Temperature, 39.2°C.; respiration, 30; pulse, 150. Patient felt no ill effects. Cover-glass preparation showed characteristic gonococci decolorized by Gram's method. In this case I was able also to cultivate the gonococcus from the urethra and isolate it in pure culture. On the ninth day cover-glass showed the same results as on the other days. Discharge was never profuse at any time. After death of patient, which occurred on the fourteenth day after inoculation, Dr. Van Gieson made a post-mortem examination of the genital tract. He will report the results of the examination.

Inoculation of the Large Diplococcus Found in the Normal Urethra.—In order to prove its non-pathogenic nature I utilized a pure culture of the eighth generation. The young man who was the subject of this experiment was seventeen years of age, and positively denied any gonorrhœal history. His urine before inoculation was clear; gelatin, agar, and chest-serum agar showed no

gonococcus. Patient was instructed not to pass any urine for the next five or six hours. Dr. Goldenberg was a witness to this experiment. At the expiration of five days his urine was examined, but with negative results; pathologically nothing could be demonstrsted.

I next endeavored to ascertain whether this diplococcus found in the catarrhal variety of colpitis is the cause of the infection. My inoculation experiments soon demonstrated that the diplococcus which I isolated from catarrhal colpitis cases is not responsible for this pathological condition of the catarrhal form of colpitis. For this experiment I selected an adult aged twenty-five, apparently in good health. Examinations of his urethra by means of cover-glass specimens and gelatin, agar, and chest-serum agar plates showed no gonococcus. A pure culture of the third generation of the diplococcus was inoculated with negative results.

Inoculation Experiments with the Diplococcus Urethræ (Turro).—Believing that Turro's supposed gonococcus had its virulence increased on an acid medium, I inoculated this diplococcus (Turro), fourth generation, from an acid gelatin medium into a healthy male urethra, as was proven by cover-glass preparations and gelatin, agar, and chest-serum agar plates showing no gonococci. The result of this experiment was negative.

Animal Experiments.—Various species of animals were inoculated with the true gonococcus (Neisser) with different results. Neisser, as early as 1882, had inoculated dogs with negative results. Loeffler and Leistikow inoculated the abraded conjunctiva and urethra of rabbits and guinea-pigs with negative results. F. Krause inoculated rabbits, cats, pigeons, and mice. Wertheim, on the contrary, claims to have produced a slight peritonitis on mice, guinea-pigs, rabbits, and rats. Finger, in his experiments on dogs, claims he produced an acute inflammation of the knee-joint of the dog by injecting a pure culture from

serum agar. He, however, obtained no reaction in the urethra or rectum of the dog by injecting the gonococcus. His experiments on the peritoneum of white mice were reported as negative. The varying results of different experimenters led me to study the effect of the true gonococcus on white mice. Two healthy mice were selected. The abdominal surface was disinfected with five per cent. carbolic solution. A pure culture of the gonococcus was previously placed in 1 c.c. of sterilized distilled water. The apparatus for injection used was the one described by Petri. A half cubic centimetre of the pure culture of the gonococcus was then injected into the peritoneal cavity. Twenty-four hours after inoculation no ill effects observed; mice eat well and appear healthy. After forty-eight hours a dissection of one of the mice was made under the control of Dr. Cheesman. Macroscopically no pathological changes could be demonstrated. Cover-glass preparations and chest-serum agar plates were made; the plates gave negative results; cover-glass specimens, however, showed a number of pus-cells with small masses of cocci which decolorized by Gram's method. To account for this we may assume that the cocci found were still the remains of the inoculation. On the other hand, again, the presence of the pus-cells may have been the starting point of a beginning peritonitis. I concluded to keep the other mouse alive for several days for further investigation. On the seventh day the second mouse was dissected, and the macroscopic appearance was the same as in the first mouse. Cover-glass preparation showed no pus-cells, and sowing on peritoneum and chest-serum agar plates gave negative results.

Experiments on Dog with the Gonococcus (Neisser). I have inoculated both eyes and the urethra of the dog with absolutely negative results. In all my experiments I was careful to transplant my cultures for the purpose of ascertaining their viability.

This, then, is a summary of my experiments and studies with micro-organisms of the normal and pathological male urethra and vulvo-vaginal tract of children. While my results have tallied in many respects with those of other writers, I have frequently found it necessary to antagonize, as you are now aware, their views and conclusions. To summarize briefly, I wil submit the following conclusions:

1. I believe that the gonococcus (Neisser) is never present in the normal urethra, as far as my experiments have shown.

2. The diplococci found in the normal urethra can positively be differentiated by the Gram stain.

3. I believe that the diplococcus described by Turro in connection with his acid media experiments is not the gonococcus.

4. I indorse Wertheim's conclusions, except that I believe the liquid sterilized chest-serum is a better culture medium in every way than placenta serum.

5. I believe that urine agar is not an ideal culture medium, as Finger claims.

6. Gram's stain is the only crucial staining test for the presence of the gonococcus (Neisser), and should therefore be employed in all cases.

7. For ordinary staining of the gonococcus I would recommend a two per cent. alcoholic methyl-violet solution.

8. Certain reports of the discovery of the gonococcus in various parts of the body, such as mouth, rectum, and serous cavities, must be looked upon with scepticism, owing to the fact that Gram's stain and culture media were not applied.

9. The normal vulvo-vaginal tract is never a habitat for the gonococcus, as far as my experiments have demonstrated.

10. Gram's stain is here also the differential stain be-

tween the diplococci found in pus-cells from the diseased vulvo-vaginal tract.

11. There is reason to believe that there is a specific micro-organism in catarrhal colpitis, either the diplococcus of Bockhart or of E. Fränkel.

12. In specific colpitis the gonococcus found is identical with the one found in specific male urethritis.

13. My inoculation experiments on the human urethra confirm the belief in the specific pathogenic power of the gonococcus (Neisser).[1]

DISCUSSION.

DR. S. LUSTGARTEN.—I have listened with extreme interest to Dr. Heiman's paper, and wish to compliment him for his painstaking work. The character of the paper is such that the greater part of it cannot be discussed or criticised without making personal researches in the directions indicated. There are just a few points to which I desire to call attention. In 1887 I published the result of my examination of the secretions obtained from the normal urethra, and among other points I stated that a diplococcus was found there which closely resembled the gonococcus both in size and appearance, and which was not necessarially differentiated from it by Gram's method of staining. I still hold that opinion, and even Steinscheider, who was a pupil of Neisser, has admitted that in five per cent. of cases there is a possibility of error. The practical consequences of this, however, are not so great, because we are, on the whole, on the safe side. That is to say, whenever we find anything in the urethral discharge which can be taken for the gonococcus, we regard the case as one of gonorrhœa, treat the patient accordingly, and advise him not to get married. Still, it interferes with the diagnostic value of the gonococcus. Of late the opinion has again been brought forward by German authorities that the male urethra may be contagious even if gonococci are not found,

[1] NOTE.—I wish to express to Drs. Prudden and Cheesman my sincerest gratitude for their kindness and many acts of courtesy extended to me during my laboratory work, and to Dr. Edward Leaming, to whom I am greatly indebted for my prints.

owing to the fact that they may have undergone involution and cannot be stained. At the present time, so far as the microscopic test is concerned, it does not enable us to give a positive opinion. As in diphtheria, we must rely upon cultures. Let us hope that Dr. Heiman's discovery of chest serum will give us a medium which will enable us to make cultures with less difficulty than heretofore. The microscopical diagnosis of gonorrhœa, in spite of the praise it has received, is not always a positive one.

DR. I. VAN GIESON.—I am very glad to have this opportunity to speak of Dr. Heiman's paper in terms of the highest commendation. His work has been careful, painstaking, and ideally judicious, and very little disposition is shown to make deductions which are not substantiated by facts. The paper embodies a large amount of work, and brings up a subject which is of great practical importance. It is only by researches such as these that the fact can be determined as to how long a patient remains infective and whether the gonococcus is present or absent in gleet. This has an important bearing upon the question of when it is safe for such a patient to marry, for it is a well known fact that a large number of uterine and tubal troubles are due to gonorrhœal infection.

The discovery of the gonococcus has been of but little practical value in the therapeusis of acute gonorrhœa in the male. The old time treatment is still considered the best. The relation of this paper to the subject of therapeusis in the later stages of gonorrhœa is one of importance. Another important feature of the paper is the stress that has been laid upon the value of Gram's method in identifying the gonococcus.

As to the persistence of the gonococcus for months or even years after the inception of a urethritis, it seems we are still unable to settle this question positively. The only way in which this can be settled is by work of just this character. It would be rather remarkable if the gonococcus would be found to persist for such long periods in the urethra, and in this respect it would differ from any other bacteria with which we are acquainted. Clinically, we have learned that in many cases of gleet there is no danger of infection.

In the case mentioned by Dr. Heiman, where I per-

formed an autopsy thirteen days after the gonorrhœal discharge appeared, the anterior urethral mucous membrane looked red—perhaps slightly more so than usual. The prostatic portion of the urethra was of a velvety hue and covered by a slight yellow film. An examination of sections with the microscope showed comparatively slight changes in the anterior urethra. The posterior urethra, however, was severely damaged. The epithelium was abraded in many places, and underneath there were collections of small round cells and extravasations of red blood cells, many of which had found their way to the free surface of the urethra. It is probable that the urethra may recover from the first attack of gonorrhœa with comparatively few changes; it is after the second or third attacks that we see the ulcers, the changes in the epithelium as well as in the glands, and the growth of connective tissue in the substance of the urethra.

I wish fully to endorse the statement made by Dr. Heiman that when the gonococci are not found, intelligent irritation of the urethra may bring them out. I have seen cases in which dormant gonococci were brought out by the irritation incident to the division of urethral strictures.

Dr. Heiman (closing the discussion).—I have very little to say in closing. I am well aware of the fact mentioned by Dr. Lustgarten that Steincheider has in five per cent. of his cases found a diplococcus which became decolorized by Gram's method. This, I think, is partly due to the different ways in which this method is carried out.

Stated Meeting, June 6th, 1895.

MEDICAL TERMINOLOGY—ITS ETYMOLOGY AND ERRORS.

By P. J. McCOURT, M. D.

Mr. President and Fellows of the Academy:

Of late, much has been said before this Academy, and much has been written in our journals, advocating the adoption of a common international language for the interchange of professional thought. However desirable such an acquisition may be, the difficulties in the way are so great that we cannot hope for its attainment during the present or even the coming generation. English will be long debarred as a common professional language, by reason of international jealousy. Greek, then, which is the basis of our nomenclature, and also the mother of modern tongues, remains the only valid language for such purpose. But an available knowledge of Greek will demand at least two years' persistent study; and there are many useful men in the profession, as well as many others about to enter it, who could never attain that knowledge. When, however, the effort has been made with apparent success, the student who was taught "Greek" in the universities of English-speaking countries will find, upon visiting Greece, or on coming in contact with scholars from other countries who have learned the language correctly, that he can neither understand nor be understood—that he will not know the pronunciation of one word in twenty. In view of these and other obstacles, it seems a waste of time, for the present, at least, to discuss a project so purely Utopian.

It should not be inferred from these premises that a knowledge of such Greek as we may learn in our own country is valueless. On the contrary, it is not only valuable but indispensable. Our terminology is derived primarily from the Greek and secondarily from the Latin. Whatever be the inconvenience of this burden, the rule has been firmly established, and we cannot now alter it. It follows that at least a slight knowledge of these languages is necessary to a comprehension of the words we use. The intelligence and dignity of the profession render this demand imperative. When a new word is required to express a new principle or to modify an old one, we take two or more Greek words from which to construct it. The word thus formed will not give a clear perception of its meaning, save its etymology be figured clearly and picturesquely in the mind of him who employs it. Ruskin long ago entered a sound and eloquent plea for picturesque reading. But many of our terms which have gained a firm hold were formed in this manner by men who were lamentably ignorant of the Greek and Latin languages; and as a consequence the Latinized Greek is often grossly perverted and corrupted. The ancient Greeks, who had not the light of modern anatomy, physiology, and pathology to guide them, gave erroneous names to many parts, functions, and diseases; and these errors have been perpetuated with but slight attempt at correction on the part of the moderns. The same people, especially the Athenians, availed themselves fully of the elastic powers of the beautiful Greek to bestow ludicrous and fanciful names upon persons and things around them; and our language of to-day is affected by the abuse of this privilege. Misnomers must have been coeval with the dawn of our science, if not antecedent to it, since the appellation of "horse-driver" was applied to one of the most gifted of ancient philosophers—a descendant of Æsculapius, the son of Heraclides, the student of Herodi-

cus, and the "Father of Medicine"—Hippocrates, from *hippos*, a horse, and *chrateo*, to govern.

Other difficulties, also, arise in the way of attaining perfection in our terminology. Aside from our yet imperfect knowledge of morbid anatomy, from the difficulty of translating idioms and of finding two or three words in which to describe the characteristic features of a disease, language itself is imperfect and often fails. Even the verb "to be," by which we declare our existence, is an imperfect verb in nearly every language having a grammatical construction. I have compared that verb in twenty-six languages, and in all of these the verb was found to be imperfect.

If, therefore, it be impracticable at this time to adopt a universal medical language, it is both practicable and highly essential to correct and improve that which we now use; and in doing this we must, in order to be understood, follow, to some extent, the erroneous pronunciation taught us. The ignoble defects of our terminology must be manifest to all who have given the subject any consideration whatsoever. Men of no other calling use such irrational and equivocative words when treating of their subjects. It is humiliating to reflect that the cowboys on the plains employ a more intelligent vocabulary for their vocation than we do for ours—one in which men are expected to clothe the highest order of human thought.

The name of a disease must be defective if it does not clearly indicate the pathological change, or the part affected, or both; or, failing these, the characteristic effects of the morbid process—and a vast majority of those in use fail to do any of these. Nor does the evil end here; for a large number of terms delude the student by indicating conditions which do not exist, even in a remote degree. While not a purist in words, or in anything else, I wish to recall to your memory a few among the more conspicuous misnomers in almost constant use, and to show, by

analysis of these, the urgent necessity of reconstructing our terminology.

Acetabulum, Latin *acetum*, vinegar, the vinegar-cup of the ancients, is a silly misnomer for the cotyloid cavity, from *kotyle*,[1] a small cup, and *eidos*, a form or resemblance.

Ague may be from the French, meaning sharp, but far more likely arises from the Gothic root *agis*, to tremble.

Allopathy, allopathist, from *allos*, other, and *pathos*, affection or disease, are terms attached, in derision, to the rational practice and practitioners of the regular profession, to signify that we cannot cure a disease without establishing another disease in its place—which we all know to be an untruth. We should never hear the terms applied without at least correction.

Alopecia, from *alopex*, a fox, instead of calvities, Latin *calvus*, bald.

Anasarca, from *ana*, through, and *sarx*, the flesh; ascites, *askites*, from *askos*, a leather sack; œdema, from *oidema*, a swelling, are all equally childish.

Angina, cynanche, from *kyon*, a dog, and *ancho*, to strangle, is shown by Pliny ("Nat. Historia," viii, 51) to have been applied to sore throat in both dogs and swine. Galen applied the term to laryngitis, while Euripides and Heraclides (Meineke's "Com. Fragmanta") before him had described the disease as *tod anchones pelas*: "'tis nigh as bad as hanging." These generic terms are now happily being superseded by others of more definite meaning.

Aphtha, from *apto*, to kindle, to set on fire—not to inflame, as rendered by Dunglison in his Medical Dictionary.

Apoplexy, *apoplesso*, from *apo*, away, from, or by means of, and *plesso*, to strike, does not tell of hemorrhage on the brain or elsewhere.

[1] Following the course adopted when touching upon this subject in 1880, I have placed the Greek words, like those of other languages quoted, in italics. The terminal *o* in the Greek words given is usually long (*ōmega*).

Artery, from *aer*, air or spirit, and *tereo*, to preserve or keep, defines a vessel in which air is the last thing contained.

Asphyxia, from *a*, privative or negative, and *sphyxis*, the pulse, describes syncope, but not strangulation, suffocation, or suspended respiration, the proper term for which is apnœa, from *a*, privative, and *pneo*, to breathe.

Atheroma, from *athera*, groats or meal, also gruel, and Atresia, from *a*, privative, and *treo*, to bore, unbored—both give false ideas of the pathological conditions they are meant to define.

Autopsy, from *auto*, one's self, and *opsis*, the act of seeing, does not necessarily imply, as many suppose it does, self-dissection. Autopsy may, without much strain, signify "seeing or examining for one's self;" but examination of the cadaver is properly expressed by the term necropsy, from *nekros*, a corpse, and *opsis*, from the future, *opsomai*, of the verb *orao*, to behold—not from *optomai*, as given in all our medical dictionaries.

The caprice of naming a disease after the author who has first or early described it is either egotistical or idiotic —egotistical when applied or sanctioned by the author, and idiotic when adopted by others. Such names give no hint of the lesion implied, and can serve only to confuse our students. As results of this folly we have Addison's disease, Bright's disease, Graves's disease, Hodgkin's disease, Ménière's disease, Scrivener's palsy, etc.

Beriberi has been deemed a term of obscure etymology, and is usually rendered from the Singhalese, to imply weakness, intensified by repetition. The verb *beri* (Aryan root *ber*) is pure Hindustani, the Braj Bhasha dialect of the Hindi, which in turn is one of the seven branches of the Aryan tongue still spoken in India. *Beri* means, in both the ancient and modern language of the Hindus, to be fettered or shackled.

Bougie, French, is no longer a "wax candle."

Bronchocele, from *bronchos*, the windpipe, and *kele*, a protrusion or tumor. Goitre, possibly from the Swiss *gutter*, probably from the Latin *guttur*, both meaning throat and equally incorrect.

Bubo comes from *bubon*, the groin, and yet is applied to certain swellings in the axillary and parotid regions.

Cachexia, from *kakos*, bad or evil, and *hexis*, a habit; diathesis, from *diatithemi*, to arrange or dispose; dyscrasia, from *dys*, bad, and *krasis*, a tempering, or temperament: are not all these misleading, incorrect, and vague?

"Cæsarean section" has served to perpetuate an historic fable, since Julius Cæsar was not cut out through the abdominal parietes. The myth had its origin in mistaken etymology: to infants thus born Pliny and others before him applied the term *cæsones*, Latin, from *cæsum*, to cut.

Catalepsy, from *kata*, intensive, and *lambano*, to seize, or to pounce upon, gives not the slightest clew to this condition.

Cataract, from *kataraktes*, to rush down (like a waterfall), might oftener be applied to fluxes than to an abnormal lens condition.

Chemists, it has been complained, are constantly changing their language; but that language is now more accurate than that of any other branch of medicine. It still retains a number of absurdities, however, which ought to be eliminated. Alcohol, Arabic, from *al*, definite article the, and *kahal*, a fine, subtle powder. Alkali, Arabic, from *al*, the, and *kali*, ash (of plants rich in soda); but kali is applied to potassium. From this source we also derive the Arabic-Greek hybrid, alkaloid: literally, "like an alkali," which the alkaloids are not. Cobalt, German, *kobold*, a gnome or demon. Carbolic acid would be better described as an alcohol. Oxygen is not the "acid-generator" which its name implies; many acids contain no oxygen, while all contain hydrogen. Ozone, from *ozo*, a stench. Sulphuric

ether—properly ethylic ether, $C_4 H_{10} O$—contains no sulphur, etc.

Chlorosis, from *chloros* (the Greek name of the element chlorine), green, is no less senseless than anæmia, from *a*, privative, and *haima*, blood, bloodless.

Cirrhosis, from *kirrhos*, reddish-yellow.

Consumption, marasmus, phthisis, and tabes each means to waste away, or to consume.

Dengue, Spanish, a short veil.

Diabetes, from *dia*, through, and *baino*, to go.

Diphtheria, from *diphthera*, skin, leather, or membrane: at best, a "disease of the membranes" (formed like the term pneumonia).

Dysentery, from *dys*, difficulty, and *enteron*, a bowel, instead of colitis, from *kolon*, the colon, and suffix *itis*, denoting inflammation of the part designated by the primitive.

Embolism, from *em*, in, and *ballo*, to throw; something or anything thrown in.

Epilepsy, from *epilambano*, to seize upon, is quite as silly as *petit mal* and *grand mal* of the French.

Epistaxis, from *epistazo*, to distil up.

Erysipelas, from *erythros*, red, and *pela*, skin. This important term is not derived in any part from *erusos*, as Dunglison renders it, since there is no such word in the Greek language, ancient or modern.

In that class of diseases which we may, with some freedom, term eruptive, there is among their names scarce one that has reason in its etymology or suggestiveness in its root. Acne may be a corruption of *akmai*, pimples on the face at puberty; or, better, menstrual dermatosis of the face; but Dunglison follows Cassius in ascribing it to *akme*, vigor— a very unfortunate condition, it must be inferred. Cancer, Latin *cancer*, a crab. Carcinoma, *karkinos*, a crab. Scirrhus, *skirrhos*, hard. Ecthyma, *ekthyo*, to break out. Eczema, *ekzeo*, to boil out. Exan-

thema, *exantheo*, to blossom out. Herpes, *herpo*, to creep. Impetigo, Latin *impeto*, to invade or attack. Lepra, *lepros*, a scaly skin. Lichen, *leichen*, moss. Psoriasis, *psora*, the itch, but applied to scaly tetter—in Europe, to the lepra of the Greeks. Rupia, *rhypos*, dirt, filth. Scabies, Latin, *scabo*, to scratch. Sycosis, *sykoomai*, to become like figs. Variola, Latin *varius*, spotted, etc. What are all these to a careful reader but misleading hints to the diseases they are meant to depict?

Gangrene, from *graino*, to gnaw. Necrosis, from *nekroo*, to deaden, to kill, is fairly accurate; and there is nothing in the latter term to restrict its application to bone tissue.

Gonorrhœa, from *gone*, semen, and *rheo*, to flow, is one of the most palpable misnomers we still tolerate. A better term would be blennorrhagia, from *blenna*, mucus or slime, and *rhegnumi*, to burst forth; or, more strictly accurate, blennorrhœa, from *blenna* and *rheo*. Baptorrhœa, from *baptos*, infected, and *rheo*, would be also admissible.

Gout, Latin, from *gutta*, a drop; podagra, from *podos*, a foot, and *agra*, a seizure, are terms whose derivations are laughable, quaint, and wrong.

Gynecology, from *gyne*, *gynaikos*, a woman, and *logos*, the doctrine of. You will remember that in the Greek and Latin languages *o* is masculine and *a* is feminine. Here we have a term purely masculine to designate a condition purely feminine. The correct term should be gynecalogy. Much akin to this paradox, but still more egregious, is the term "Mariolatry," coined by a bigot of divinity to imply worship of the Virgin—which no church teaches.

Hernia, from *ernos*, *hernos*, a branch or offshoot, is certainly misleading.

Hydrophobia, from *hydor*, water, and *phobos*, dread or fear, so named from one of its occasional symptoms, instead of the condition, which is that of rabies, Latin *rabio*, to be mad or to rage. But the correct term is lyssa, from

lyssa, rage, madness; also the worm under the tongue of dogs, believed to produce madness (Pliny, XXIX, 32).

Hypnotism, from *hypnos*, sleep.

Hypochondriasis, from *hypo*, under, and *chondros*, a cartilage, gives, vaguely, a locality where is an organ which may be, at times, the cause of the condition announced in the misnomer.

Hysteria, from *hystera*, the womb, should have immediately been discarded for a more rational term when well-marked cases were first observed in the male. Yet, in view of this familiar fact, Dr. Barnes has proposed a name nearly as vague as the old one—"oöphoria," from *oon*, ovum, and *phero*, to bear—based upon the hypothesis that the ovaries and not the uterus are the seat of this neurosis.

Hybrid terms, each formed from two languages, were justly denounced by the ancient Greeks as *hybris*, lawless, violence, wantonness. This mode of word-building, at once constructive and destructive, is a vice which even ignorance cannot excuse. As examples of these hybrid terms we have albuminuria, from Latin *albus*, white, and Greek *ouron*, urine. Fibroid, from Latin *fibra*, fibre, and Greek *eidos*, appearance or resemblance. Terminology, from Latin *terminus*, a term, and Greek, *logos*, a discourse or treatise. Tuberculosis, from Latin *tuberculum*, tubercle, and Greek *osis*, denoting formative process, etc. We might with as equal propriety say throatitis as vaginitis.

Idiocy, from *idiotai*, private citizens, not office-holders.

Influenza, Italian, influence (of the stars).

Laparotomy, from *lapara*, the loins, and *tome*, a cutting or incision; properly, incision for nephrectomy, and not abdominal section, the correct term for which is cœliotomy, from *koilia*, the abdomen, and *tome*.

Laryngismus, from *laryngizo*, to shout loud or often. The secondary meaning of spasm sometimes attached to this suffix cannot redeem the error.

Lithotomy, from *lithos*, a stone, and *temno*, to cut. As Campbell naïvely points out, it is the bladder, not the stone, that is cut; and he suggests the correct term, lithocystotomy.

Lymph, Latin *lympha* (from Greek *lymphe*), clear water.

Malaria, Italian, from *mala*, bad, and *aria*, air, illustrates how a number of diseases have been named from their causes instead of their effects.

Malignant, Latin *malus*, bad or evil, is but little worse than its equivalent, virulent, Latin *virus*, a poison, which, in turn, is derived from Sanskrit *veshas*, a filthy poison.

Mania, from *mainomai*, to rage—akin to *men*, the moon, and equivalent to the Latin *lunaticus*, from *luna*, the moon—are examples of ancient superstitions to which we still cling.

Melancholia, from *melas*, *melanos*, black, and *chole*, bile, is not remarkable for lucidity.

Myopia, from *myo*, to shut, and *ops*, the eye.

Neurasthenia, from *neuron*, a nerve, and *asthenia*, absence of strength—although the nerves would seem to have acquired, or at least transmitted, all the strength and activity of the body.

Nymphomania (see gynecology), from *nymphe*, a maiden, and *mania*, should give place to andromania, from *aner*, a man, and *mania*. *Nymphe* cannot be translated by the word clitoris, as Dunglison has done, and nowhere in history has the noun been so used.

Pancreas, from *pan*, all, and *kreas*, flesh, might have been applied with some excuse to the misnamed prostate (from *pro*, before, and *istemi*, to stand), which contains but little if any adenoid tissue.

Paralysis, from *para*, beside, and *lysis*, from *lyo*, to loosen.

Paresis, from *pariemi*, to relax, or to misdirect.

Phlegmasia dolens, from *phlego*, to burn, or *phlegmazo*,

to inflame, and Latin *doleo*, to be in pain; either but poorly describes what ought to be called obstructive phlebitis.

Phrenitis, from *phren*, the mind (properly, the midriff or diaphragm, also the heart, but never denotes the brain), and *itis*. Encephalitis, from *en*, in, *kephale*, the head, and *itis*, has been more generally sanctioned; but meningocerebritis is less faulty than either.

To treat of the "physiological" action of drugs upon the body, instead of their pathological, pathogenetic, or toxicological action, is an abuse of language.

Pneumonia, from *pneumon*, the lungs, and terminal *ia*, denoting a morbid condition of the organ named: a "disease of the lungs," should be replaced by pneumonitis, or even pulmonitis.

Polypus, from *polys*, many, and *pous*, foot.

Quarantine, Italian, from *quaranta*, forty (days of exclusion).

Ranula, Latin dim. of *rana*, a frog.

Rheumatism, from *rheuma*, a flux, catarrh, or diarrhœa, merely implies an indefinite morbid humor in the body, but gives no intimation of change in muscular, articular, or synovial tissues.

Scorbutus, Teutonic, from *schaar*, torn, and *buuk*, the abdomen.

Scrofula, Latin *scrofa*, a sow with many young; correct only by inference that the disease is common to swine and to those who eat of their flesh.

Surgeon, surgery, from *cheir*, the hand, and *ergon*, work or exploit—although the essence of the "exploit" is brain work.

Syncope, from *syn*, intensive, and *kopto*, to cut, to cut short.

Syphilis; "etymology unknown" or "uncertain" is the comment of most lexicographers. That given by Blancardus—*syn*, together, and *phileo*, to love—was for some time accepted by many. But there is little room to doubt

that its true derivation is from *sys*, a swine, and *phileo*, as given by the poet-doctor Francastoro in his medical poem, "Syphilidis sive Morbi Gallici," libri tres, Verona, 1530, one of whose characters, a swineherd, contracted the disease. But neither term describes the lesion.

Tetanus, from *teino*, to stretch; so named, doubtless, because the condition is just the opposite, that of muscular contraction. Trismus, from *trizo*, to grate or gnash the teeth, is equally faulty.

Tic douloureux, French, painful twitching.

Nowhere does greater chaos reign than among the names of fevers; but we shall only consider that of typhoid, from *typhos*, stupor, and *eidos*, a form or resemblance; literally, "like typhus," which it is not. Enteric fever, from *enteron*, an intestine, is equally inappropriate. The term suggested by Murchison, pythogenic fever—*pythogens*, from *pytho*, putrid, and *gennao*, to generate, "born of putrinity"—while not correct, is the best yet offered.

Zymotic, from *zyme*, leaven, or from *zymosis*, a ferment. Both forms are, like many of the others given, so ambiguous as to call for no criticism.

Let us now compare the foregoing jargon with terms which are technically correct, which clearly describe the lesion implied, and the contrast will be worthy of note.

Endocarditis, from *endon*, within, *kardia*, the heart, and *itis*, denoting inflammation.

Hæmatemesis, from *haima*, blood, and *emeo*, to vomit.

Hydropneumothorax, from *hydor*, water, *pneuma*, air, and *thorax*, the chest.

Retroflexion, Latin, from *retro*, backward, and *flecto*, to bend.

Is it not possible to render the whole, or nearly the whole of our terminology as accurate, beautiful, and explicit as this? It would seem that the time has arrived when such advance can be made, and that the profession

is ready to accept it. Scholars and men of science should no longer be forced by ancient rule or modern error to speak in childish prattle, nor to perplex themselves and those who are to follow them by the use of terms which falsify their meaning. The reconstructive process, however, cannot be intrusted to the compilers of medical dictionaries. It is not the lexicographer who makes a language, but the scholars who speak and write it. Hence the reformative effort, to achieve success, must be collective and made in unison, and not merely individual. If this Academy, as the leader of professional thought on this continent, will take the initiative, its action will be readily seconded by medical journals and college professors; and soon our terms will be made to reveal ideas which they now conceal, pervert, or obscure.

From this mongrel tree of philological blunders I have plucked but a few of the withered leaves. I could wish that the office had found one who was also competent to do the pruning. That portion of the work will need for its execution the united efforts of expert pathologists and Greek scholars—and I claim no place among either.

DISCUSSION.

DR. C. A. LEALE.—The subject of medical terminology, its etymology and errors, offers to the mind material for apparently unlimited work. Scientific men of all nationalities have, at various times, endeavored to solve the problem, and no gatherings of our profession have shown such intense longings to attain definite results as have been exhibited to us at the different international congresses, where the learned men of our profession, speaking in a great variety of languages, have at times utterly failed to convey their ideas; and nothing causes a deeper sense of individual humiliation than for a scientific man to sit and listen to the observations of another in his own profession without being able in anyway to comprehend his meaning.

Again, we meet with another difficulty, which opposes

the possibility of our ever attaining a universal language, namely, the great varieties of nations, climates, soils, and surroundings. The experience of man has shown that where a people, having a comparatively pure language, has been transplanted to a place where the surroundings are entirely different, and complete isolation prevents communication, the language formerly spoken undergoes a change, which in three or four generations makes a large portion of it incomprehensible to those in the mother-country.

The Chinese represent one of the oldest cultured people inhabiting the world. They have a written language for their four hundred and three million inhabitants, yet the spoken dialects of their numerous provinces are so varied as to be incomprehensible to a large proportion of those living in even the neighboring provinces. This fact was forcibly illustrated in the large masses of troops from the different provinces, under one flag and one general, representing the mother-country, yet differing so in the language they used as in many cases to fail in understanding the ordinary military orders. Again, we refer to our own American Indians, and we find that of the two hundred and fifty thousand inhabiting the United States, although they have been in comparatively intimate contact for many centuries, each tribe speaks in a dialect unknown to the others. These uncultured people bring to their aid signs, looks, and gesticulations, which are more universally known than any written or spoken tongue. The language of the deaf-mute is specially adapted to his purpose, and as modified and improved by the best teachers, illustrates a universal method of conveying the idea of desire, pleasure, pain, joy, or sorrow, more generally understood by those who use different languages than any tongue could convey.

In regard to the best international language for the interchange of professional thoughts, I still believe that English will, year by year, gain the ascendency. Not by reason of any power of scientific people, but more in consequence of the conquering spirit and universal distribution. The means by which wealth may be more easily acquired, the necessary interchange of thought for commercial purposes and colonization, compel the inhabitants of far distant lands to acquire a knowledge of the language of those

who come to conquer, add wealth, build new cites, and eventually rule. International jealousy under the pressure of an overwhelming power fades as a mist before us. Only the powerful can hold their own, and might makes right in the dealings of the civilized with barbarous people.

In regard to the pronunciation of words spelled the same in different parts of the civilized world, with people speaking the same language, we find a difference so great as to make the word almost unrecognizable. The word phthisis is pronounced among English-speaking people in four different ways; some call it theesis, some teesis, others tisis, or thisis. Still another perplexity to the student is the terminology of such words as pneumonitis, bronchitis, peritonitis, and cerebritis. These and other words receive such varied pronunciations from teachers in different parts of the country as to be so characteristic of the localities from which they come as to enable us in many instances to identify the college in which the individual was educated.

The English language being complex, to understand it thoroughly one must have a knowledge of Greek and Latin. Like the Greeks, we also have the means to coin new words.

How few of our accomplished Greek scholars of to-day are able to converse with educated Athenians! To learn Greek one must live in Athens.

There are other difficulties in the way of perfect terminology, which seem impossible to overcome. If words be coined to represent morbid anatomy, we not only need to express our meaning to-day, but advances to-morrow may so completely overturn the supposed cause of the disease as to make the word based upon the pathological lesion of to-day absolutely incorrect when the new thought is desired to be recorded. This one argument alone shows how unattainable it is for us to secure even an approximate international language for the profession of medicine, which in the next decade could stand unchanged. Besides, the same morbid anatomy may manifest itself in an entirely new way when influenced by very great change in the surroundings. If a plant can be so entirely changed by transportation to a different climate (as the flax of Ireland, which runs almost to fiber, and is nearly inert; or to the Indian hemp, which

in that climate generates the poisonous hashish), how much more susceptible to change will mankind always be when we acknowledge his extreme sensibility to impressions?

We now reach that part as to the practicability and highly essential importance of continually making an effort to correct and improve the language we now use.

I cannot agree with the essayist when he states that it is humiliating to reflect that the cowboy on the plains uses a language which more correctly conveys his impressions than ours does for us.

We can easily descend to the lower orders, and mention the sounds used to convey their meaning. The roar of the lion, the bark of the dog, and the singing of the different birds, each conveys its individual impression. We have been informed recently that even in inanimate nature we have harmonies such as apparently defy the understanding of man. The roar of Niagara has been recently stated to be composed of perfect harmonies. Even the change that is constantly going on in vegetable cell-life is productive of a harmony which never changes.

The caprice of naming a disease after the author, who has first or early described it, is correctly stated to be either egotistic or idiotic; as the Smith disease, the Brown disease, or the Jones disease, which conveys as little intelligence as the term Bright's disease or Graves's disease.

Chemical terms are much more accurate than those of any other branch of medicine. We must remember that here also we are supposed to deal with exact known quantities. Yet even here during the last few months we have been informed that a chemist has discovered in our atmosphere a new elementary substance named Argon. Notwithstanding the vast quantity of our atmosphere and its previous analysis by chemists in all parts of the known world, we are now told that it contains an additional substance.

This shows us that even what we suppose to be exact knowledge to-day, may to-morrow prove to be so inexact as to necessitate a change in the name of the substance. It also shows us that there is no end to knowledge, and so long as there is no end to knowledge there will be no end to the necessity of a change of terms to make that knowledge comprehensible.

For more than one hundred years we have had in our

own country diligent men in the medical profession, who have endeavored to improve medical terminology.

The subject has been presented to the American Medical Association and other medical societies, committees have been formed, and united efforts have been made to reach a basis upon which future building of medical words may be made.

The efforts of Bard, of Drake, of Flint, of Clymer, and later, but more prominently, of Gouley, have perpetuated their names as medical terminologists.

DR. D. LEWIS.—I freely confess at the outset my inability to discuss a paper of this character. The first impression I received while listening to it was the great care and labor and energy that Dr. McCourt has expended in its preparation. The importance of the subject I cannot question, but, as Dr. Leale has said, it would be impossible to adopt a terminology which would apply next year in every respect. Some palpable errors which exist could and should be remedied, but in the field of nervous diseases and of skin diseases, and in fact, in all departments of medicine, a new pathology is developing from year to year, to say nothing of the diversities of opinion among pathologists themselves, which, to my mind, forms an almost insurmountable barrier to the adoption of a strictly accurate and permanent medical terminology. After all, coming back to the principles of language, if a certain word expresses a certain idea which we understand, is not that as good a language as we need? If I know what is meant by the term "cancer," for example, if the word gives me a clear idea of what the speaker intends to convey, then that word is just as useful and instructive to me as it would be if its primary etymology and scientific application were strictly correct. If the line of investigation made by Dr. McCourt was followed in other directions, outside of medicine, we would find terms employed which are just as incongruous or irrelevant as the word "cancer," or the various other examples which the doctor has cited in his paper.

DR. W. H. THOMSON.—I fully appreciate both the learning and the motives exhibited by the writer of the paper, but the question he raises is certainly not one which we can discuss. It is a fact that medical termi-

nology resembles very much some rocks which the geologists tell us were formed by the dropping of water holding in solution certain mineral matter, and containing certain delicate forms of plant life. The process of forming these rocks was so gradual that even these delicate objects retained their form and beauty. So we must remember that the ideas presented in these old and now inaccurate terms represented notions which have had their day, which have died, and which have become entombed in these words. The important question is, Are these words leading us astray? The word "pneumonia," it seems to me, conveys as clear an idea to our minds as do many common words. For example, we know what is meant by "calico," even though we do not know that it originally referred to a fabric made only in Calcutta. The same may be said of the word "damask," which originally meant a fabric made in Damascus.

We cannot employ with advantage words indicating the pathology of a disease, because our ideas of pathology are constantly shifting. For example, we are now told that parenchymatous nephritis is always interstitial, and that interstitial nephritis never has its being without being also parenchymatous. I do not see, therefore, that we can expect to have our terminology correspond to our pathological opinions.

I think, however, we can find some important guiding principles. One of these is to eliminate from our terminology all names of diseases based on symptoms. Only a very short time ago I saw in consultation with a physician a case of locomotor ataxia. The attending physician was astonished at my diagnosis of locomotor ataxia, because the patient had presented up to this time no marked motor symptoms. There had been, however, dysuria, with gastric and laryngeal crises. In this disease, the motor symptoms are often so extremely insignificant that it is unfortunate that the name locomotor ataxia has been given to it. Again, I have often diagnosticated exophthalmic goitre in cases of extremely rapid action of the heart, tremor, and other symptoms pointing to the condition, yet in which physicians had hesitated to make the diagnosis because of the absence of exophthalmus. Here is another example of a name obscuring the disease. Again, we should adopt the

term "infectious" to denote any disease dependent upon the introduction of an organism into the body. The notion that tuberculosis is a contagious disease has been responsible for much hardship, whereas the proper use of terms would have avoided such serious consequences. If we could revise the terminology of these diseases we would confer a great boon. It is also important that by concert of action a disease should be given only one name, instead of a number, as is often the case at present. The reform in our terminology should not attempt to disturb names whose signification is perfectly distinct and universal, simply because of their improper derivation. If we should go to such lengths as these, we would probably only cause worse confusion.

It is extremely discouraging to note at the present time the tendency to the reckless coining of terms, particularly in neurology. For instance, the word "neuron," although only three years old, already has six different meanings. I think we can say that the terms which have been based on modern pathology are even worse for the variety than those introduced by the ancients.

DR. A. ROSE.—I beg to differ with Dr. Thomson in regard to his remark that we should not use terms denoting only symptoms. We cannot do better in some instances. Thus Dr. Einhorn, for instance, has coined the term *achylia gastrica*, which, although signifying only a symptom, is the best term we can use, for in some instances the different anatomical or pathological changes causing the affection cannot be discovered at all, not even by autopsy; in other instances, only an autopsy may reveal the cause. Many hybrid words would not be introduced into our terminology, or better, "lexicology," if a better knowledge of living Greek were more common, if the terms were not made with the aid of the lexicon only. Thus we would not have the word serumtherapy, as we find it in German literature, but orotherapy instead. For the same reason, there would not be endless controversy about some terms, as, for instance, about the spelling of the word polyclinic. No Greek can ever think of spelling "policlinic". Polis means city, but if a Greek would say cityclinic, he would always say astyclinic (asty, city). We can write "astyclinic" or "astypolyclinic," or "polyclinic" simply, but never "policlinic." There are many eminent college-

Greek scholars in our profession who have coined and are coining words which they suppose are Greek, which the genuine Greeks, however, call hellenophanis (ἑλληνοφανεια) because they only seem to be Greek words.

DR. McCOURT (closing the discussion).—In treating of the terms analyzed in the paper, I have endeavored to be critical, but not hypercritical. If we continue to use terms like pneumonia and rheumatism, because they are well understood, we furnish an argument for the continued use of almost all of the other misnomers I have cited. It is in this way that historical errors are handed down and perpetuated. I would like to emphasize one great objection to the use of these terms as they now stand: They are so untrue that we cannot employ them under oath in a court of justice without doing violence to our conscience; we must explain the terms or stultify ourselves. This fact alone is sufficient to demand their expulsion from our language.

The Latin being a dead language, we do not know how it was pronounced by the Romans; but as the Italians were known to have followed the Romans very closely in their pronunciation, and as they sound the "a" broad, and the "i" like "ee," we should follow this method of pronunciation. In the Greek there is no "i" sound except in the diphthong ει, when two points are placed above the "i" to indicate that sound. Erroneous teaching is responsible for the various mispronunciations of such terms as the one we call "teesis." P or ph at the beginning of a Greek word, when followed by n, s, or t is not silent, as we have been taught—never. This term—phthisis, a wasting, from *phthio*, to waste away—is, like nearly all Greek words, pronounced just as it is spelled, every letter being sounded. Any other pronunciation is purely arbitrary, and is either a personal whim or a local habit. I am glad that Dr. Thomson has spoken of the importance of watching the coining of words, and preventing the introduction into our language of improper terms.

Stated Meeting, October 3d, 1895.

ABSTRACT OF A REPORT OF THE GYNECOLOGICAL SERVICE AT MOUNT SINAI HOSPITAL FROM JANUARY 1, 1883, TO DECEMBER 31, 1894.

By PAUL F. MUNDÉ, M. D.

Mr. President and Gentlemen:

In May, 1882, I was appointed gynecologist to Mount Sinai Hospital, to succeed Dr. Emil Noeggerath, resigned. For seven years previously I had been gynecologist to the dispensary connected with the hospital and my appointment, therefore, was in the line of promotion. During the twelve years from January 1, 1883, to December 31, 1894, there were admitted to the gynecological service under my charge 4,211 patients; of these, 2,211 were discharged cured, 1,456 improved, 191 unimproved, and 102 died. Three hundred and thirteen were discharged after examination, their complaint not being gynecological.

My service in Mount Sinai Hospital comprises twenty free beds, with two rooms for the treatment of capital operations, and my choice of twenty-five private rooms for which the patients pay the hospital a fixed rate. The service is a continuous one, and only by means of the permission of the hospital authorities to take a vacation during the summer months has it been possible for me to stand the strain of so continuous an application to professional work.

I shall only touch upon the most interesting features of the cases observed by me during the twelve years included in this report, and I shall begin with the external organs

and proceed inward in the natural, anatomical order. It will be my object, of course, to emphasize certain mooted questions of diagnosis and treatment in order, if possible, to draw out the opinions of my audience.

The first lesion of interest which I find mentioned in the report is that of a curious injury of the hymen which I saw in three cases. This consisted in a tearing loose of the whole ring of the hymen from its vulvar attachment, with the exception of one point about half an inch in diameter. The hymeneal ring was thus allowed to hang loosely over the vaginal orifice, and of course coition underneath was attended with no difficulty, but the patients all complained of occasional pain during coition, which was located at the external orifice and I had no other explanation than that the male organ accidentally caught in the normal opening of the hymen and not being able to enter the vagina, dragged upon its attachment and caused the painful symptoms. My attention was first called to this anomaly by the report of a Swiss physician who gave a very graphic description of this accident and its possible results.

One hundred and eighty-four cases of laceration of the perineum were admitted to my service; of these one hundred and twenty-one were incomplete and sixty-three complete. One hundred and forty-one were cured by operation, forty-one were improved only, the operation not proving an entire success; but all cases which were thought worthy of treatment were operated upon. Up to the year 1886 the teachings of the Woman's Hospital school of operative gynecology had influenced me to such an extent that I closely followed the methods of Emmet and Thomas in all my operations upon lacerated perinea. I made the old butterfly-shaped denudation, extending more or less up into the vagina if there was a rectocele, and used only silver-wire sutures which were carried under the whole denuded surface, exactly as described by Emmet. I will not deny that my results in those cases were exceedingly good, even

in instances of complete laceration, but I still had a few failures after operation of the latter accident, and the operation was in itself long and somewhat difficult. In the summer of 1886 I chanced to see Mr. Lawson Tait in Birmingham perform the so-called flap-splitting operation for complete laceration of the perineum. He did it so rapidly that only those who stood close by could see what he did. I was fortunate enough, however, to be one of these, and on my return home I determined to give this new operation—as I then thought it—a thorough test. It is, of course, well known that Lawson Tait was not the originator of the operation; he simply revived it, and the original operator is still under discussion, and you undoubtedly all know the operation differs from the old methods in that there is no tissue whatever removed, but that the recto-vaginal septum is split transversely, the incisions extending up on to either labium if the laceration is incomplete, or down on either side of the anus if complete. Each flap is then drawn up or down respectively and the sutures are introduced transversely. The principal advantage of this operation is that it takes but a very short time to perform it, irrespective of the fact that no tissues are removed, and I must say that so far as its results are concerned I have been very much better pleased with it than with the old operations of Emmet and Thomas, or of the more complicated ones for complete laceration of Simon and other older operators which I used to employ. Since 1886 I have done this operation certainly from thirty to forty times and I have not seen a failure. I employ silkworm gut and the Peaslee needle. Practically I may say that I have discarded silver wire for all my perineal operations, not that it would not be as good a suture as any other, but because it causes more pain after the operation and during its removal than silkworm gut and has no advantage whatever. In all perineal operations I have for many years followed the rule of first having the bowels thoroughly evacuated

for several days previous to the operation, then allowing them to remain quiescent under fluid diet for at least four days after the operation, and then having them moved by laxatives every day until convalescence is completed. Constipation of the bowels after operations on the perineum is no longer practised.

Although not strictly gynecological, affections of the rectum have been frequently witnessed by me in the female, the reason being that the symptoms were attributed to some disease of the sexual organs proper, whereas I found that they were due entirely to the lower bowel. The most common complaint of this kind is catarrh of the rectum which I have found to cause backache, bearing down, throbbing, and painful defecation so frequently that unless I find a very good cause for these symptoms in the sexual organs proper, I make it a rule to examine the rectum before concluding my diagnosis. A very satisfactory examination of the lower part of the rectum can be made by turning it out with two fingers in the vagina. Besides, the ordinary cylindrical specula or the Sims specula, with the patient in the knee-chest position, will afford a very free view of the bowel up to the level of the sacral promontory. I have thus discovered a catarrh, ulcer, and stricture of the rectum in many cases and been enabled to apply the necessary remedies through these media. For catarrh of the rectum my treatment has been to first dilate the sphincter ani thoroughly and then swab out the bowel with a solution of nitrate of silver, one drachm to the ounce, being careful to keep the walls of the intestine apart by filling it with melted vaseline or warm sweet oil, or even introducing a small cotton tampon. The daily injection of from two to four ounces of warm Carron oil (equal parts of linseed oil and lime water) has always seemed to me an excellent remedy in these cases of inflammation of the lower bowel. Ulcers of the rectum I have usually treated with the direct application of nitric acid.

Diseases of the bladder are, of course, very frequently met with by the gynecologist. One of the most common affections is urethral tenesmus which may depend upon merely a functional disturbance of the neck of the bladder or upon actual cystitis. In either case I have found dilatation of the urethra, such as can be performed in the office with or without the assistance of cocaine, a very efficient remedy which at least relieves symptoms and even might cure the condition. Of course, where decided bladder symptoms presented, a thorough examination of the urine, chemically and microscopically, was essential to a correct understanding of the case. In cases of chronic cystitis, where the bladder mucosa was shown to be decidedly diseased, I have found that internal medication that is per os, was of very little benefit. I have seen considerable relief from infusions of uva ursi and triticum repens and from sandal-wood oil, but the irrigation of the bladder once or oftener daily with a 1-1,000 solution of boracic acid or Thiersch's solution has seemed to me indispensable to an improvement, and in old cases where the bladder mucosa was diseased I have found nothing but the formation of an artificial vaginal fistula to afford relief. This fistula might remain open for six months or a year, but should never be closed until all the bladder symptoms have been entirely and permanently relieved. Occasionally I found that it was necessary to scrape out calcareous deposits on the walls of the bladder through the dilated urethra or an artificial vaginal fistula.

Thirteen cases of fistula between the bladder and the sexual organs are reported; two of these were between the uterus and the bladder. In one I was obliged to stitch the posterior lip of the cervix to the remaining portion of the vesico-vaginal septum, thus turning the uterine canal into the bladder. Twenty-six stitches were required to thoroughly approximate the denuded surfaces. A cure was the result. Of course this alternative is an unpleas-

ant one; but under the circumstances there was nothing left except to close the vagina completely by the operation of elytrorrhaphy which would in no way have benefited the woman, since she would have to menstruate through her bladder, and being a married woman, sexual intercourse was impossible. The formation of a vagino-rectal fistula, which has been practised in Germany as a substitute for elytrorrhaphy in extensive destruction of the vesico-vaginal wall, certainly is not a desirable operation. I left the woman with a vagina about three inches deep, and so far as previous experience of others goes, the bladder very soon adapts itself to the irritation of the menstrual discharge. The other case of utero-vesical fistula died from the accidental rupture of an ovarian abscess which was brought about by the traction employed during the effort to approximate the edges of the fistula. Of course the existence of such an abscess was unsuspected and only revealed at the post-mortem. Of the other eleven cases of vesico-vaginal fistula nine were cured, one improved, and one discharged uncured owing to refusal to submit to operation. I employed the Sims method of operation, namely, shallow paring of the edges of the fistula and non-inclusion of the bladder walls in the sutures. Silver wire was used.

One of the most common affections of the vagina was that of prolapse of the posterior vaginal wall together with the anterior wall of the rectum known as rectocele. It occurred in 110 cases and whenever considered of sufficient consequence was treated by operation. It may be as well to consider rectocele together with the prolapse of the anterior vaginal wall and bladder or cystocele, since both of these conditions very commonly occur together, and if requiring operation are treated at the same sitting. In my opinion, if rectocele is unattended with a decided laceration of the perineum, and if the uterus is displaced backward at the same time, as is very apt to be the case, it may

be possible to relieve the symptoms by simply replacing the uterus and keeping it in position by a properly fitted pessary. The replacement of the uterus will in such cases usually, more or less, result in a restoration of the prolapsed posterior vaginal wall, but if the perineum is torn and the posterior vaginal wall very much relaxed, no pessary will be retained, and then only a narrowing of that portion of the vagina by operation will give relief. Of course the cure of the rectocele does not in itself bring about the cure of a retroversion or retroflexion, therefore a pessary may have to be worn even after the rectocele is cured. I am decidedly in favor of curing a pronounced rectocele by operation, and the method which I have employed during the last nine years is that accredited to Professor Hegar, of Freiburg, where I first saw it done. It consists in making a triangular denudation of the posterior vaginal wall, extending almost up to the cervix and on either labium as high up as it is desired to build the new perineum. The edges of this denuded surface are then brought together by a continuous interlooped (mattress) catgut suture until the vaginal orifice is reached, when deep silkworm-gut stitches are employed to close the external wound. These latter are passed so deeply as to catch up the separated fibres of the levator ani muscle. I first saw this operation performed by Wiedow, Hegar's son-in-law and assistant, with silver wire stitches which of course had to be removed within a given time. This was always attended with some difficulty, and hence I have employed with unvarying benefit the continuous catgut stitch just described. I prefer this operation to the old-time rectocele operations of Emmet and Thomas and to the new operation of Emmet which I have performed a number of times very successfully, but still I think so far as the formation of á solid posterior vaginal wall and a sound perineum are concerned the Hegar operation is superior. So far as cystocele is concerned, I dò not

operate as often as I do on rectocele, for the reason that unless there is a complete prolapse of the whole vagina, when both anterior and posterior walls have to be narrowed, the simple cystocele operation is not so successful in its permanent results as that on the rectocele. This is quite natural, since the greatest pressure upon the lower portion of the trunk of a woman in the rect position is exerted upon the anterior vaginal wall with the bladder, and the cystocele is therefore most likely to return. It is for this very reason, that is the failure of the cicatrix to hold under the steady downward strain exerted upon it, that I have given up the horseshoe, oval, and other forms of denudation for this condition and have limited my operative treatment for cystocele entirely to the circular denudation with tobacco-pouch suture recommended by Stoltz, of Nancy. This operation is quite as easy of performance, and the advantages are that the stitch, being tied in one knot just below the meatus urinarius, can be removed with one clip of the scissors or even be allowed to cut through when it has done its duty. Thus any interference with a perineum which may have been operated upon at the same sitting is not needed. I therefore always employ this operation of Stoltz's when I think it necessary to narrow the posterior vaginal wall or repair the perineum at the same sitting. In cases where only the anterior vaginal wall with the bladder is prolapsed, the uterus being in normal position and the vagina not too much relaxed, I have found the pessary devised by Dr. Gehrung, of St. Louis, to answer the purpose so well of keeping in place the prolapsed vagina and bladder that I have not thought it necessary to perform a plastic operation. I have patients under my care now who for years have been wearing this supporter, calling upon me only every two or three months for its removal and cleansing. One old lady even, over seventy, comes miles for this purpose only, and all say they could not possibly get along without the support.

I find fifteen cases of stenosis and atresia of the vagina reported in which I restored the canal to its normal calibre or made a new one. In one instance both uterus and vagina were double, one side being open, the other closed. Menstruation of course occurred from the open side, but the menstrual blood was retained in the closed half, and the distention of that portion of the genital tract eventually caused pain for which the patient came to the hospital. I made the diagnosis and excised the septum between uterus and vagina, making a single canal from vulva to fundus.

Two cases of epithelioma of the vagina were observed in which, contrary to the usual rule, the disease originated on the posterior vaginal wall, the cervix uteri being entirely healthy. This location of the disease is rare, since it was met with only twice in over sixty cases of cancerous disease of the sexual organs. I found it impossible to eradicate the disease, being obliged to limit my efforts entirely to the curette and caustic which of course gave only temporary relief.

Of all the diseases of the uterus which came under observation the most common was that of laceration of the cervix. I am almost afraid to bring up again this hackneyed subject—it has been literally run into the ground during the last ten or fifteen years—and still I feel that there is no other not absolutely dangerous condition of the uterus which is more productive of discomfort to the woman and more susceptible to intelligent treatment and cure than laceration of the cervix. I find 518 cases recorded, of which 342 were operated upon by the method of Emmet, with 316 recoveries. In 160 cases an operation was not considered necessary; the sharp curette, iodized phenol, and nitric acid being thought sufficient for the relief of the symptoms. I can only repeat in this connection what I said as long ago as 1879, in an article which I read before this very Academy on the indication of hystero-trachelorrhaphy, namely, that a lacerated cervix requires opera-

tion not so much for the laceration itself as for the symptoms which that laceration produces, whether it be large or small. Thus a small laceration which causes a cervical endometritis with hyperplasia of the cervical glands, or one that keeps up a subinvolution or hyperplasia of the uterus, may need operative repair, whereas a much larger one which is not accompanied by these symptoms can be safely left untouched. In former years it was my custom to remove the silver-wire sutures at the end of a week or ten days, being careful to keep the patient in bed and quiet during the interval, but a very unpleasant experience in two instances caused me to change this rule; for on removing the stitches at the usual time in both cases the freshly united edges of the wound parted and I was obliged, under various subterfuges, to scrape them raw and introduce fresh sutures, not wishing to tell the patients of the failure of the operation. I directed them to leave their beds in a few days and report at my office after the first menstrual period, when I found to my surprise and delight that union was complete and permanent. In fact, I rather believe that patients would do perfectly well after operations for lacerated cervix if they were allowed to get up and go about their usual employments; for I am sure that absolute rest in the recumbent position is not at all necessary for the union of the lacerated lips, but self-preservation obliges us to insist upon rest after this operation, since if it failed we would certainly be blamed for not having taken every necessary precaution, and would probably not have the opportunity to repeat the operation, not to mention the possibility of a loss of the expected fee. My practice, therefore, has been of recent years to direct all patients upon whom I have operated for laceration of the cervix, to remain in bed for a week after the operation, then get up, go about their usual avocations according to their feelings, and report at my office after the following menstrual period, when I have usually removed the stitches. I have had no

occasion to regret this change of practice. I can truly say that in properly selected cases I have seen more benefit result from operation of a lacerated cervix than from any other treatment of any one of the minor lesions of the female sexual organs, and this benefit has not been confined entirely to the pelvic organs, for I have seen most decidedly favorable results in the cure of general anemia, hysteria, and other reflex neurotic disturbances. One other indication for the operation for lacerated cervix I have not referred to, and that is the possibility, and not a very remote one either, of a malignant degeneration of the raw, bleeding surface of a large laceration of the cervix. I do not think that we ought to allow patients whom we can influence sufficiently to undergo the operation to take the risks of such a future contingency.

How many of the fifty-four cases of epithelioma of the cervix which I find recorded, were due to laceration of the cervix, I do not pretend to say; judging only from my recollections of hospital and private practice, I should say that in fully two thirds of the cases of epithelioma of the cervix, the women had borne children and had, therefore, probably suffered from more or less laceration of the cervix. In only three of the fifty-four cases was I able to achieve a cure. In one of these a very severe hemorrhage during the examination prevented my paying attention to the condition of the body of the uterus. The woman complained of having flowed profusely for three months, and I found a soft polypoid growth attached to the anterior lip of the cervix. This I removed by the galvano-cautery wire, and it was found to be a well-marked epithelioma. Two months later she returned for examination by request and reported that she had not menstruated since. Examination showed the uterus enlarged to the size of the fifth month of pregnancy and distinctly containing a fetus. The cervix was entirely healthy. A year later she returned with the child in her arms, and the cervix was then still

found to be normal. In the other two instances the galvano-cautery wire was also employed and the disease apparently entirely cured, for when seen again, about six months later, the cervix was still healthy. Of the remaining fifty-one cases, all I was able to do in forty was to remove as much of the diseased tissue as possible with the sharp curette and chloride of zinc. This treatment of course was only palliative and could not be expected to prolong life more than a comparatively short time. Still in one instance the patient lived four years after the curetting, which was performed only once. In eleven cases the entire uterus was removed per vaginam, but I am sorry to say, in no instance, with a permanent recovery; in fact, out of twenty-six cases of vaginal hysterectomy, with three deaths, which I have performed, I have not seen a single instance where the patient remained free from the disease for a period longer than nine months after the operation. In no case has life been prolonged more than two years by the complete extirpation of the uterus. I suppose that I have been exceptionally unfortunate in my experience. Probably, for some to me unknown reason, I have failed to see the cases early enough to enable me to effect a permanent cure by the removal of the diseased organ, but I have certainly seen several where I felt perfectly sure that so far as my finger and eye could determine, all diseased tissue was removed with the uterus, and still a recurrence took place. I confess that my faith in the curability of cancer of the uterus by hysterectomy, whether vaginal or abdominal, has not been sustained by my results. The same applies to the six cases of extirpation of the uterus for malignant disease of the body—four epithelioma and two sarcoma. Of these latter all the patients recovered, but the disease returned. I have no explanation to offer and can simply state my experience, and still I should think it my duty, whenever I feel that I can completely remove the diseased uterus, whether by the vagina or by

the abdominal section, to do so and give the patient a chance. I am perfectly aware that vaginal hysterectomy has been very much more successful as regards the recovery of the patient from the operation than the abdominal operation, and still I cannot help feeling that with the patient in Trendelenburg's position, and a thorough survey of the field of operation and the possibility of protecting by towels the rest of the abdominal cavity from infection, the supra-pubic method of complete hysterectomy is by far the more surgical and technically superior procedure. I do not feel that my own experience justifies me in attempting again to remove a cancerous uterus, unless a thorough, careful examination, if necessary under anæsthesia, has convinced me that the uterus alone is involved and that the parametrium is absolutely free from disease. I do not see the use of subjecting the patient to the danger, discomfort, not to mention expense, of a hysterectomy, whether vaginal or supra-pubic, for the removal of her cancerous uterus unless there are at least seventy-five chances out of a hundred in her favor for a permanent cure. Acting on this principle, I have refused many an operation which I have good reason to believe has afterward been performed by others who could not possibly have had any reasonable expectation of curing the patient. Many cases of cancer of the cervix, indeed, in which the patient does not bleed and has only a comparatively trifling discharge,—suffers no pain, in fact, hardly knows that she is afflicted with an incurable disease—many such cases, I repeat, should, I think, be left alone. The disease progresses more slowly, I am sure, if it is not interfered with, and when the case is at once recognized to be incurable, what is the use of inflicting unnecessary pain and discomfort upon the patient? Simply palliative remedies will in such cases, it is my honest conviction, do much better for the patient than doubtful or hopeless operative procedures.

I may as well say here, that so far, I have not happened

to meet with a case where I could conscientiously perform the now so popular operation of extirpation of the uterus, per vaginam, for diseased appendages and pelvic suppuration. I do not deny that I may see such a case at any time, indeed, I had one in my service last spring where after a comparatively simple celiotomy for adherent tubes and ovaries, for some unknown reason, weeks after the recovery of the patient, a diffused pelvic imflammation with large exudate set in, with the eventual breaking down into pus of one portion of the exudate after the other. As I was about going on my summer vacation I turned the case over to the assistant gynecologist, Dr. Brettauer, with the remark that if I were to remain on duty I should certainly do vaginal hysterectomy on this case and open all the pelvic sinuses and abscess pockets. Dr. Brettauer performed this operation in two sittings and told me that he found it exceedingly difficult, the patient almost succumbing from secondary hemorrhage and requiring saline infusion,—however, she eventually recovered entirely. I do not, therefore, question the justifiability of vaginal extirpation of the uterus for suppuration of the appendages and pelvic tissues in properly selected cases, but my experience certainly leads me to regard such cases as not very common, as rather the exception than the rule in diseases of the adnexa and pelvic inflammation, and I cannot help questioning the judgment of surgeons who report with pride several hundred such operations performed by them during the last three or four years with but trifling mortality. It seems to me that these gentlemen are riding a hobby as fascinating as it is likely to be ephemeral; for I believe some leading German operators—Leopold, of Dresden, for instance—who surely are not timid with the knife, are already calling a halt on this indiscriminate and reckless vaginal slaughter of the uterus. A few more years will doubtless put this operation where it belongs, that is, in the position of a most excellent method for dif-

fuse pelvic suppuration which resists less radical measures, but not to be recommended for chronic endometritis or diseased appendages which can be safely removed by celiotomy.

Endometritis.—It will be seen by the large number of cases reported, 297, that this disease is of a very frequent occurrence. Indeed only such cases were admitted to the hospital as could not safely be treated in the office or clinic. The chief significance of chronic non-puerperal endometritis is the production of sterility, and it was for this reason mainly, and also for the irritating discharge, that the cases were admitted. The treatment consisted in dilatation and curetting of the uterine cavity, followed by the thorough application to the endometrium of a fifty per cent. solution of chloride of zinc in the worst cases, and of a solution of iodized phenol, equal parts, in the milder instances. A sterilized gauze drain was then inserted through the internal os, the patient put to bed, and all precautions taken against inflammatory reaction. A repetition of the cauterization with milder solutions, if thought best, usually resulted in a permanent cure in the course of two or three weeks. My figures show 197 cures, with 94 cases of improvement out of 297 operations, only six being mentioned as discharged unimproved. I am sure that none of the milder methods of treatment such as can safely be practised in the office, would be followed by so large a proportion of beneficial results. Of course the best hope for a permanent cure of chronic endometritis will result from impregnation and normal delivery.

Fibroids.—One hundred and thirty cases of fibroid tumors of the uterus were admitted to my service, the large majority of which were of the body, only in three instances was the fibroid growth contained entirely within the tissue of the cervix and was removed by enucleation and traction through the vagina. In one instance the patient was pregnant in the fifth month and the rupture of the membranes

and prolapse of the umbilical cord necessitated immediate action. As no other course remained than Cæsarean section except the enucleation of the tumor and delivery of the child through the vagina, I chose the latter method and with great difficulty succeeded in removing a tumor weighing three pounds; the child and placenta were then easily delivered. The other two cases occurred in virgins, and both the cervix and perineum had to be split in order to enable the delivery of the tumor which was effected by the ordinary obstetric forceps. Cervix and perineum were repaired at a subsequent sitting. In twenty-one cases the fibroid tumor had developed into a polypus and was removed with comparative ease. In twenty-nine instances abdominal hysterectomy was performed, with twenty-five recoveries. The extra-peritoneal treatment of the stump was adopted by me in all my cases up to within the last two years, since which time, however, I have employed the intra-peritoneal method which I was able to do easily in the Trendelenburg position. I preferred to leave in a small portion of the cervix which I covered completely with peritoneum and thus closed entirely the abdominal cavity. I have used this method in preference to the ex cision of the cervix because I thought that it gave better results as regards the formation of a solid pelvic floor. All my operations performed in this manner have recovered, and three of them are not included in this report, as they occurred since January 1st, 1895. In removing fibrous tumors which have become polypoid, per vaginam, I have always been careful to incise the capsule of the pedicle as high up as I could reach it and to enucleate the upper portion of the tumor before cutting off the capsule, fearing that I might possibly injure the uterine wall which is apt to be inverted by the natural downward growth of the polypus and particularly by the traction exerted upon it during its removal. In several of my cases the differential diagnosis between inversion of the uterus and polypus

was exceedingly difficult owing to the combination of partial inversion of the uterus produced by the polypus. In forty-six cases the symptoms produced by the fibroids were improved in course of time by ergot, curetting, and intrauterine galvanization, that is, the hemorrhage, pain, and growth of the tumor were controlled for the time being. In three cases galvano-puncture of the fibroid was practised through the vagina and a current up to 150 milliamperes passed through the tumor and uterus for fifteen minutes under anæsthesia. Only one sitting was given in each case. This treatment was followed by quite extensive inflammation and sloughing, but gradually the tumor disappeared and all three patients were discharged cured. In very nearly one-half of all cases of fibroid the symptoms were not thought sufficiently severe to warrant any dangerous operative procedure. It may be well known that I am very conservative in my views as to the necessity for the removal by abdominal section of fibroid tumors of the uterus, inasmuch as such tumors in themselves seldom cause death. I have felt that I was not justified in subjecting the patients to an operation which even with our present wonderful technique does at times endanger their lives, unless the symptoms produced by the tumor were such as at the time or in the near future were likely to prove serious. If the tumor was known to be growing rapidly; if the hemorrhages could not be controlled by curretting, ergot, hydrastis, and galvanism; if the pressure symptoms produced by the tumor, rendered the patient's life miserable; if she was in more or less constant pain; if attacks of peritonitis or inflammation of the uterine appendages had occurred and were liable to occur again, or if, finally, the patient absolutely insisted upon being relieved of her abdominal enlargement, under all such conditions, I certainly would feel myself justified in removing the uterus and tumor together with the appendages; but I have never felt that I had a right to perform this operation on

the mere possible contingency of future unpleasant results arising from the tumor, for it is well known that a large proportion of patients with fibroid tumors never suffer from them at all, would not even know of their existence unless informed by a physician who happens to discover them during an examination made for some other pelvic or abdominal symptoms, and further that a not inconsiderable number of such tumors gradually shrink and disappear of their own accord. To resume, therefore, my practice has always been to advise the expectant treatment in all cases of fibroids which did not call for immediate surgical interference, but I have been careful to recommend to such patients that they present themselves for examination two or three times a year, or at any time when a sudden increase in the growth of the tumor or new alarming symptoms manifested themselves. I confess that I have been immensely gratified by the numerous cases of fibroids of the uterus, particularly in private practice, which have followed this conservative advice of mine, and have returned after one or more years with the tumor entirely *in statu quo* or even diminished in size and its possessor in perfect health. No matter if every patient recovered from an abdominal hysterectomy for fibroid, why perform the operation unless it is really necessary? I have seen great benefit result from the persistent use of galvanism in fibroids so far as the arrest of the growth and even its shrinking are concerned. I have within the past year seen two cases, one after fifty, the other after twenty-four, galvanic sittings administered by the family physician by my advice, where the tumor had diminished certainly one half. It goes without saying, of course, that any fibroid which becomes polypoid or grows downward in the vagina and can be enucleated through that canal should be thus removed. My above remarks apply only to those fibroids which grow upward into the abdominal cavity.

Discission and dilatation of the uterine canal for con-

genital constriction was performed ninety times, the object being mostly to cure sterility. The canal was kept open by a hard-rubber stem in former years, lately by packing with iodoform gauze. What the results were as regards the cure of the sterility I am unable to say, since such patients seldom return to report. Of retroversion and retroflexion there were 161 cases with 85 recoveries, 66 improved, and 10 unimproved. The recoveries were mostly due to the performance of Alexander's operation and a few to ventral fixation; the improvements, to the retention of the uterus in the normal position by pessary. I have already placed myself on record so thoroughly as regards Alexander's operation for properly selected cases of retroversion and retroflexion, that I need only repeat that I still prefer it to any of the other methods of radical cure of that displacement of the uterus. I have performed it seventy-three times, as given by this report, in all about eighty times, and with but very few failures. I admit its technical difficulty, but I claim that a failure to find the ligaments is always the fault of the operator. So far as I have been able to trace the cases, the recoveries have been permanent, and the dangers have been absolutely *nil*, since I have never lost a case and seen no other bad effects than an occasional suppuration of the incisions. I twice performed the intra-abdominal shortening of the round ligaments with good success, having been obliged to open the abdominal cavity in those cases for the detachment of the adherent uterus and appendages. If the uterus is movable and the appendages normal, I certainly should never think of doing anything but Alexander's operation. For prolapse of the uterus I do not favor the shortening of the round ligaments alone, since they are not sufficiently strong to keep up permanently the prolapsed organ, but I always associate with the Alexander operation the anterior and posterior narrowing of the vaginal canal and perineorrhaphy. Twice I operated for the cure of prolapsus uteri

et vaginæ by the new method introduced by Herman Freund, of Strassburg, which consists in inserting a series of circular thick wire sutures round the prolapsed mass, beginning at the external os and replacing it step by step as each suture is twisted. The results, so far as I can judge, were perfect. Of course the vagina is almost entirely closed, the stitches being left in as permanent splints.

Only one case of inversion of the uterus was observed of one year's duration, in which repeated efforts made before admission, and by myself afterward, failed to replace the organ. The same result followed abdominal section and persistent effort to stretch the ring of the inversion with glove stretchers according to Thomas's method. I was finally obliged to remove the ovaries, close the abdominal wound, and apply an elastic ligature to the pretty well mulitated uterus, the result being a sloughing of the organ and a complete recovery. I would hardly recommend this procedure, but under the circumstances saw no other way out of the difficulty.

Hysterorrhaphy or ventral fixation of the fundus uteri was performed ten times for retroflexion and twice for prolapsus, with one death—the case of prolapsus. I cannot conscientiously favor this operation for a condition which in no way endangers life.

Fallopian Tubes and Ovaries.—Salpingitis, acute or chronic, more or less frequently associated with chronic oöphoritis, occurred 651 times, with 75 recoveries after removal, 41 cured after local treatment without operation, 512 improved after palliative local treatment, 22 unimproved, and one death after operation. I have been exceedingly conservative and careful in formulating the indication for removal of the appendages diseased in consequence of more or less remote inflammation. It would have been very easy for me to have operated on probably 500 of these 651 cases without putting my conscience to a

very dangerous tension, but I have always felt that while it was very easy to remove the appendages, it was never possible to restore them, and there being only two to each woman, it was but fair to give her the benefit of every possible chance for their restoration to health by less radical treatment. I may err on the wrong side in my conservatism in this respect, but I do not believe that many women have seriously suffered through the delay in the removal of their appendages for which I may have been to blame. When it comes to actual distention of the tube with pus, true pyosalpinx, the case is quite different, for out of the forty cases recorded, I removed twenty-six by celiotomy with twenty-one recoveries, and cured eleven others by vaginal incision and drainage. As a rule I prefer to remove every pus tube by abdominal section unless it is firmly adherent to Douglas's pouch, is unilateral and can be easily and safely opened and drained through the vagina. If both appendages are diseased I would never think of doing anything but their removal through an abdominal incision. I have not as yet been able to accustom myself to the new method of removing the appendages per vaginam, though I do not say that I may not in time adopt it.

In fifteen cases the Fallopian tube was removed for pregnancy, both before and after rupture; thirteen of these patients recovered. I am no longer a convert to the employment of galvanism for the arrest of tubal pregnancy, although I had a successful case of this kind about twelve years ago. In one additional instance I omitted to remove the ruptured tube, although I made the diagnosis of ruptured ectopic pregnancy, because the hemorrhage was arrested and I did not see the case until late in the evening. During the night a secondary hemorrhage took place and before an operation could be performed the patient died. The post-mortem verified the diagnosis. I have always regretted my action in this case and it has been a lesson

to me for the future. Still I have seen a fair number of cases in which all the history and symptoms clearly indicated a ruptured tubal pregnancy, in which a boggy mass was felt in Douglas's pouch, and the indication for immediate celiotomy seemed perfectly clear. The apparent cessation of the hemorrhage, the patient's good condition, and the absence of preparation for an immediate operation justified its temporary postponement, and such patients being carefully watched recovered without any surgical interference, the pelvic tumor being gradually absorbed. It is exceedingly difficult in my opinion to lay down positive rules for the guidance of the surgeon in every such case. Unquestionably the only proper course is, if the diagnosis is reasonably certain and the blood not encapsulated, to open the abdomen at once and remove the ruptured tube. In one of my cases the rupture had taken place at least a month previously, there was general peritonitis, and the abdominal cavity was filled with foul decomposed blood, the temperature being 104°. I made the diagnosis and operated within half an hour after seeing her. It seemed impossible that such a case could recover, and still under careful treatment and thorough vagino-abdominal drainage recovery did take place. The ruptured tube being easily found and removed, there could be no doubt of the accuracy of the diagnosis. It was one of the most desperate cases I have ever seen recover after an abdominal section.

Celiotomy for ovarian tumors and cysts of the broad ligaments was performed 149 times. Of these 128 were ovarian tumors with 11 deaths, the rest comprised hematoma, carcinoma, papilloma, and acute œdema of the ovary, and 7 instances of cysts of the broad ligament. The rate of mortality, a little over 8 per cent., in the cases of ovarian tumors is not high when we consider the desperate character of some of the cases when admitted. Two cases of fibroid of the ovary were operated upon, with recovery.

In 25 cases the ovarian tumor was dermoid, 10 being of the right, 10 of the left, and 5 on both sides. One of the tumors contained a switch of hair which I here present to you, which is I think almost unique in medical literature in regard to size. This was a case of double dermoid, the other tumor containing a smaller tuft of hair with teeth and bones. In 23 cases torsion of the pedicle of the tumor was found. With one exception all these recovered. In 10 instances the right ovary was at fault, in 13 the left. The direction of torsion was almost invariably from the affected side; that is, if it was a cyst of the right ovary the twist was toward the left, if of the left ovary toward the right. The cyst was thus found usually on the opposite side from its origin. It is curious that so decided an interference with the nutrition of the tumor as was found in some of these cases was attended with comparatively little constitutional disturbance and that recovery was so uneventful after the operation. The nutrition of the tumor by adhesions would to a great extent account for this. In 16 cases celiotomy for abscess of one or both ovaries with occasional pyosalpinx was performed, with 15 recoveries.

A number of celiotomies were performed for other pathological conditions of the abdominal organs, such as tubercular peritonitis, general purulent peritonitis, sarcoma of rectus muscle, displaced kidney, perityphlitic abscess and for exploration which I cannot spare time to touch upon here. Twice I removed the ovaries to check the growth of fibroid of the uterus and relieve pain and hemorrhage, with perfect success.

I have still two subjects to discuss and one is *pelvic peritonitis and pelvic cellulitis.* Pelvic peritonitis was by far the most common disease in my service, there being recorded 602 cases, with 304 recoveries, 281 improvements, 10 discharged unimproved, and 7 deaths. This proportion of mortality, about one per cent., is what I am accustomed to see from pelvic peritonitis; in general peritonitis

of course the proportion is rather the other way. The attack may last a long time, even for months, and still after a time the exudate completely disappear and apparently the pelvic organs be restored to their normal condition. I say apparently, because it is doubtful whether the appendages, particularly the tubes, are ever entirely healthy again after a severe prolonged attack of pelvic peritonitis and the reason of this is probably that in a large majority of the cases the disease has its origin in acute salpingitis, whether puerperal, infectious, gonorrheal, or not. The worst prognosis as regards ultimate restoration to entire health is certainly found after gonorrheal infection. Pelvic cellulitis occurred far less frequently, only 79 cases being noted, with 67 recoveries, 8 improvements, and 4 not benefited (discharged by request); no deaths. With rare exceptions these cases of pelvic cellulitis followed parturition. The course of pelvic cellulitis is shorter and less turbulent than that of pelvic peritonitis, and a perfect restoration to integrity of the pelvic organs far more common. The differential diagnosis between these two conditions is by no means easy and I do not here pretend to enter into a discussion of the subject. Suffice it to say that where there is no pelvic peritoneum there can be no pelvic peritonitis, and where there is no pelvic cellular tissue there can be no pelvic cellulitis.

My treatment has usually been absolute rest, opium only to relieve pain and in as small doses as will effect this object, a thorough clearing out of the bowels to begin with, and then regular but easy evacuations every two or three days. If the temperature goes above 102° and the patient's strength is fair, cold or ice to the abdomen; if under 102° or the patient depressed, hot applications; when the temperature has gone down nearly to the normal rate and the exudate only remains, large blisters over the abdomen with hot poultices; only after the temperature has become normal should general tonics, particu-

larly iron, be given. The patient is not allowed to sit up in bed until at least a week of freedom from pain and absence of temperature has elapsed, not to leave her bed until another week, and not to walk about for another, since relapses are very liable to occur from indiscretions. What the future of these cases, particularly pelvic peritonitis, will be cannot easily be foreshadowed. Undoubtedly many of them come under the knife of the abdominal surgeon sooner or later for the removal of the suppurated or adherent appendages. Still I have no doubt that many also recover their health more or less completely and never require any surgical or other treatment.

In 103 cases the pelvic exudate underwent suppuration, with 87 recoveries, 6 improvements, one not improved, and 9 deaths. I do not pretend to say in how many of these cases the abscess was originally intraperitoneal or sprang from a pelvic cellulitis; certainly when I operated on the patients the pus was shut off from the peritoneal cavity. In 95 cases the abscess was opened by lateral abdominal section, in quite a number of these cases the abscess was opened through the vagina also, and in some others only vaginal puncture was employed with drainage. It has always been my practice to open all pelvic effusions of blood or pus at the spot where the mass pointed most prominently, whether that was in the vaginal roof or through the abdominal wall. By preference I have opened per vaginam when I could do so, but of course a large proportion of these cases do not enable us to employ this natural drainage channel.

But one subject still remains to me and I have done, that is, pelvic hematocele and hematoma. By hematocele I understand an effusion of blood into the pelvic peritoneal cavity, whether free or closed off by adhesions above. By hematoma I mean an effusion of blood into the pelvic cellular tissue, mostly between the layers of the broad ligament. Practically both conditions are the same when

once the hematocele has become encapsulated. Probably all such effusions of blood, whether intra- or extra-peritoneal, are due to the rupture of an ectopic pregnancy, mostly tubal. When the effusion of blood is free in the peritoneal cavity and appears not to have reached its limit I have already said that abdominal section is the proper course; but when the blood has become encapsulated or is extra-peritoneal and bulges deep into the vagina I have found its evacuation through the vaginal roof, no matter where it may originally have started from, to be a perfectly safe and efficient method. Out of 11 cases of hematocele treated in this manner I had 3 deaths, and of 16 cases of hematoma 2 deaths. I do not believe that the results after abdominal section in the same kind of cases would have been nearly as good. Whether a ruptured ectopic pregnancy was the original cause of the hemorrhage I could not determine in any of these cases, since there was no trace of fetus or embryonic tissue to be found. I am aware that I differ in my method of treatment from some of my colleagues, but I am willing to stand upon my record. I have not seen a case of intra-pelvic effusion of blood which extended so high up out of the pelvis as to require a lateral flank incision to evacuate the blood, although I can readily imagine that this might occur.

It has of course been impossible for me in this brief abstract to give more than superficial points on many subjects which I would have liked to discuss more in detail, and I am aware that by these omissions I may lay myself open to criticism which will often be found by my critics to have been forestalled when they come to read the whole report in print.

Allow me to recapitulate the operations performed on the different organs:

On the vulva	16
On the perineum	186
On the rectum	29

On the urethra and bladder			41
On the vagina			173
On the uterus, cervix	397		
body	516		913
On the ovaries and tubes and miscellaneous			409
Total number of operations			1,767
Celiotomies (medium incision) there were			357
Laparotomies (lateral incision)			107
Total abdominal sections			464

In conclusion, to show the increase of the service from year to year there were admitted in 1883, 181 patients and 47 operations were performed; in 1894, 505 patients, with 295 operations, or an average per week of over five operations on each weekly operating day throughout the year. I have never been a victim of the "furor operations," and have always sought to cure my patients without resorting to the knife whenever possible. This is particularly true of the elective operation of removing the diseased uterine appendages, which I could have performed hundreds of times had I chosen, for there certainly was no lack of opportunity. If I erred on the side of excessive conservatism in this regard, there were plenty of other operators ready to correct my error.

DISCUSSION.

DR. R. A. MURRAY.—I do not wish to take up any time in discussing this most excellent paper. I simply wish to ask Dr. Mundé what his experience has been with trachelorrhaphy before and after the use of the curette? When trachelorrhaphy was first performed there was no curretting of the uterus nor dilatation of the cervix. To-day almost every one who does a trachelorrhaphy at the same time disinfects and curettes the uterus, and necessarily dilates the cervix (to cure the endometritis and cause involution of the uterus). I would like to know whether there has been any difference in his results since curretting of the uterus has been practised?

DR. J. BRETTAUER.—At the present day, when we hear

and read so much about extirpation of the uterus and its appendages, it is refreshing to listen to a paper on conservative work, such as we have heard to-night. In the main, I agree most heartily with the statements made by Dr. Mundé, and I see nothing to criticise in his paper. Of course there are some points regarding which Dr. Mundé has his own views, and his results certainly go to show that there are several ways to arrive at the same end.

DR. P. E. HARRIS, of Paterson, N. J.—I have been very much interested in this paper, and there is one question I would like to ask Dr. Mundé. I would like to know whether, in his operations for narrowing the vagina at the anterior wall—an operation which I think he has advocated a good deal in times past, and which he has performed many times for prolapse of the bladder—he has ever, in one or more instances, in any way injured the ureters? If I understand his method of operating correctly, he removes very considerable areas of tissue. I have performed the operation a number of times myself, and although I have never met with the accident mentioned, I have always been afraid that it might occur. I have not always been able to locate those organs.

DR. C. E. QUIMBY.—I would like to ask a question of Dr. Mundé. In a recent article on the subject of prostitution published in one of the medical journals the statement was made that a vast number of cases of suppurative disease of the tubes were of gonorrheal origin which had caused no vaginal inflammation due to weakened gonococci. I should like to ask Dr. Mundé if in any of his cases of tubal disease a bacteriological examination was made to determine the presence of gonococci in patients who had had no gonorrheal discharge?

DR. H. L. COLLYER.—I have seen a great deal of Dr. Mundé's work, and was much interested in his paper to-night. Judging from the statistics of other operators, the only criticism I can offer is that he has been a little too conservative. He has lost many opportunities to swell his statistics, as other operators have done.

I am glad to hear that Dr. Mundé has come to the conclusion that electricity will not accomplish all that has been claimed for it. I believe it will produce absorption

of inflammatory exudates, but that it has little or no effect on organized tissue.

To Dr. Mundé we are indebted for much of the progress that has been made in gynecology in recent years, especially in the treatment of endometritis by the sharp curette. He was an advocate of that instrument when almost every one else condemned it.

DR. A. JACOBI.—In order to prove my gratitude to Dr. Mundé for his excellent paper I would suggest that he write another, embodying certain of the points he has touched upon to-night.

In his paper, Dr. Mundé spoke of the use of chloride of zinc in endometritis. I know that this has been used a good deal for this purpose, also that it has been strongly objected to. I have used it in many cases, and while it often did great service, I found that in many instances, especially in the region of the cervix, it gave rise to cicatricial contractions which were very troublesome. I would like to know whether Dr. Mundé has had a similar experience. He also spoke of bichloride of mercury in the treatment of chronic parametritis. I have used this a good deal and it has often rendered good service. I found that this treatment could be resorted to with perfect safety, even in patients whom I saw very seldom—once every month, or six weeks, or two months. It has been my practice to prescribe the drug in small doses and interrupt it from time to time. I prefer the bichloride to the iodide because it is better digested and does not give rise to abdominal pain. The dose was usually one quarter of a grain, daily, given in pills in divided doses between meals. This was continued for a week and then the treatment was interrupted for a time. In cases where a speedy effect was desired, I alternated between mercury and the iodides, perhaps giving the mercury one week and the iodides the next.

The subject which I hope Dr. Mundé will treat in a second paper is this: that he will utilize some of the vast amount of material he has at his command and tell us what, if any, relation these pelvic diseases bear to other organs in the body—to the heart, lungs, etc.; what influence they have on the general health of the woman, and, on the other hand, what is the etiology of these diseases. We all know that the relation between diseases of the

colon and those of the uterus and its appendages is a very close one; the relation between heart and lung diseases and those of the abdomen and pelvis is also very intimate, and I have no doubt that Dr. Mundé, who has seen so many of these cases, and who has studied them so attentively, will be able to give us some very interesting views on this subject.

DR. MUNDÉ (closing the discussion).—In former years it was my practice to curette the uterus at one sitting, and then, at a subsequent operation, after the endometritis was supposedly cured, I sewed up the cervix. I have changed that now, because I think it is an unnecessary infliction to keep the patient in bed for so long a time as is required by the operations done separately. I now curette the uterus and at the same time sew up the cervix. My cases get well quicker, and in that respect, at least, have done better.

In reply to Dr. Harris's question I would state that I have never injured the ureters in doing the cystocele operation, because I do not go deep enough into the tissues.

In my opinion, cases of pelvic disease due to a collection of plastic lymph, or so-called coagulated blood-serum, do not run along for years. They usually get well in the course of three or four months. When, on the other hand, you have chronic inflammation of the tubes and ovaries, with old fibrous adhesions, such cases cannot, I think, be influenced by any treatment by the mouth. The knife is the only resort.

As Dr. Jacobi has said, the objection to the chloride of zinc treatment in endometritis is that we may go too deep. Still, I have employed it very often, and if we do not make the applications too strong we will not get cicatricial contraction of the uterine canal.

Stated Meeting, October 17th, 1895.

ON THE QUESTION OF NON-INTERFERENCE IN ABSCESS OF CHRONIC TUBERCULOUS DISEASE OF THE JOINTS.

A CRITICAL ANALYSIS OF THIRTY-FIVE CASES OCCURRING IN THE SERVICE OF THE NEW YORK ORTHOPŒDIC HOSPITAL.

By NEWTON M. SHAFFER, M. D.

There is probably no subject which has come before the Orthopædic Section of this Academy, or the American Orthopædic Association, which has occasioned more discussion or a wider difference of opinion than that which involves the treatment of tuberculous abscess of chronic joint disease. In both of these bodies the matter has been widely discussed, and many statements have been made regarding the treatment of abscesses; but there have been no actual statistics presented of the results obtained. On one occasion—the fifth annual reunion of the American Orthopædic Association, held in Washington, D. C., in connection with the Congress of American Physicians and Surgeons, in September, 1891—I ventured to read a paper, On the Benign Course of Abscess in Pott's Disease under Efficient Mechanical Treatment.[1] The discussion which followed was interesting and instructive. But the writer found himself, as he has on some other occasions, confronted by a somewhat impulsive and dogmatic expression of opinion. As usual, there was on either side scarcely more than a mere expression of opinion—there were no

[1] *N. Y. Med. Jour.*, February 20, 1892, and *Trans. of the Amer. Orthopædic Association*, Vol. iv, p. 178.

statistics. It was under these circumstances that the writer promised that he would treat all the patients with tuberculous abscess in the New York Orthopædic Hospital upon the plan of non-interference, using only efficient mechanical treatment and constitutional remedies, and that he would make a formal statistical report of the results; the question being: Which is the better way to treat the ordinary cold abscesses of tubercular joint disease: shall they be freely incised with the usual aseptic precautions, or shall they, under ordinary circumstances, be allowed to follow their own course practically unmolested?

I suppose to many minds this question seems absurd—so strongly is the operative side of the question intrenched in the minds of the general surgeon. But a long experience has brought with it to the writer certain conclusions which differ from the conventional practice of the day, and it is with the hope of throwing some light upon the subject that I stand here to-night to redeem the promise made to my colleagues of the American Orthopædic Association over four years ago.

In order to place the matter fairly before my hearers it seems only right that I should make some extracts from the essay above referred to—that essay being, so to speak, my text for this evening.

After some preliminary remarks, I say:

"I feel that surgeons generally stand as a unit on the subject of the surgical treatment of *acute* abscess; and if the *chronic* abscess in tubercular disease resembled the *acute* abscess in its more important particulars, we should not hear the animated discussion that always follows the introduction of the question of the treatment of abscesses arising from tubercular joint disease. And I think it is correct to assume that we are all too apt to regard 'abscess' in its generic sense rather than in its pathological sense; that we are too apt to regard a fluctuating tumor, containing the products of a chronic, infectious disease, as

a something to be got rid of at almost any cost, ignoring the fact that the contents of a tubercular abscess differ in many important respects from the contents of an abscess due to acute, non-tubercular lesion. How many of us would hesitate about the propriety of opening a well-marked acute perityphlitic abscess? How many of us would deliberately open a chronic intrapelvic abscess due to a chronic and progressive tubercular disease of the dorso-lumbar spine? The extremists who would open every tubercular abscess connected with an actively diseased spine or joint are, I think, as much in error as those who ignore the indications which point to surgical interference.

"My results—after I have adopted the plan of opening every chronic abscess—were not so satisfactory as those which followed non-interference. It is true that at that time the antiseptic method of Lister was not available, and the tubercular bacillus had not been discovered by Koch. After the antiseptic method was introduced I followed it very closely, but still I found my results in chronic abscess of joint disease were not satisfactory. It seemed to me that in opening a tubercular abscess from Pott's disease for example, we were treating a 'symptom' rather than the disease; that we were tapping a reservoir and paying but little attention to its source; and that we were in too much haste to give exit to the so-called 'pus,' which kept on flowing from the source, notwithstanding our external antiseptic dressings. It seemed that high temperature would frequently develop, notwithstanding the gauze and protective irrigation and drainage, and that repair, as a rule, was delayed rather than promoted by our efforts to find a short route to recovery.

"After an experience among many cases and many methods, I came gradually to adopt a course which appeared to me to have a rational foundation. I attempted to insure as perfect a mechanical protection as possible to

the diseased joint or spine, and to maintain the general health in every available way, hoping that the actual disease might cease before the abscess opened (and this proved to be the case on many occasions), or to await the occurrence of either severe local or important general symptoms, due to the abscess itself, before I resorted to incision, etc. After I adopted this plan, I found to my surprise that many abscesses entirely disappeared; that some became quiescent or encysted; that few, if any, gave rise to trouble; that those which opened spontaneously almost uniformly did well; and that my results were more satisfactory and more permanent."

Among the cases I could submit to illustrate this position I cite the following:

Master S. W., aged eight years, consulted me on March 4th, 1887, bringing a letter of introduction from Dr. Weir Mitchell. The patient had Pott's disease of the spine, involving the eleventh and twelfth dorsal vertebræ. There was a slight kyphosis which had evidently been progressing for over a year, but which had been unnoticed until it was discovered by Dr. Mitchell. The patient was pale and thin and in a generally bad condition, but gave a good hereditary history. The prominent feature of his case was the presence of three large abscesses, one in the pelvic cavity, another in the gluteal region, and a third on the inner aspect of the thigh—all on the right side. Fluctuation could be detected between the femoral and pelvic abscesses. The gluteal abscess seemed not to be connected with the others. Both the gluteal and femoral abscesses were very large, the former being especially prominent, while the latter increased the circumference of the thigh three inches over the measurement of the thigh of the unaffected side. The pelvic abscess extended nearly to the free border of the ribs. The patient had only a slight rise of temperature, the daily evening temperature averaging 99.6°, the morning temperature averaging 98.2°, for a period of over a month during which the record was kept. The analysis of the urine showed nothing abnormal, and all the vital organs were in good condition.

An antero-posterior support (a modified Taylor's) was carefully adjusted, though at first it seemed a difficult thing to do on account of the gluteal abscess, which was so large and extended so high up that it interfered with the hip band of the apparatus. Special provision being made for this, the apparatus was adjusted and the patient was closely watched. He went to his home in May after ten weeks of careful treatment, during which there was a very perceptible decrease in the size of all the abscesses, and in July, 1889, they had wholly disappeared. Careful attention was paid during treatment to the general health and to the climatic surroundings of the patient. He did not spend one day in bed on account of his spinal disease during the entire treatment. I saw the patient during the present summer (1891), and he is well and strong and as active as many boys of his age. As he was so active, I advised that a very light apparatus be worn as a matter of precaution simply, though I have not regarded the patient as being under treatment since May, 1890. The curvature has not increased.

On November 5th, 1890, Miss J. B., of Brooklyn, aged ten years, consulted me, bringing a letter of introduction from Dr. Samuel T. Hubbard. The patient had Pott's disease of the first and second lumbar vertebræ, with abscesses very much like those existing in the case just related—viz., large intrapelvic, gluteal, and femoral abscesses, the two last named being especially large and prominent. The patient did not have any rise of temperature above the normal. The antero-posterior spinal apparatus was applied, and the patient was brought to me from Brooklyn every week. Constitutional remedies were used, and the patient was instructed to go out of doors every pleasant day. At the end of two months there was a perceptible decrease in the size of all the abscesses, and at this date (September, 1891) fluctuation can not be detected at any point, and the patient is in remarkably good health. The apparatus is still worn, and the patient is still under professional observation. The kyphosis has not increased.

Both these patients have completely recovered without any increase of deformity. The first named, the boy, is

at boarding school, playing football; the second, the girl, is well and going to school, exercising in all respects like her playmates.

I have operated on several patients in similar conditions, and I have seen other surgeons operate in abscesses of this class. The results have been unfortunate. In almost every instance a prolonged chronic septicæmia has followed, with death as the result in the majority of cases, and this is particularly true of adults and adolescents.

On the other hand, under the plan of non-interference, with efficient mechanical treatment, I have seen many happy recoveries, of which the two patients reported above are only examples, and in the adult and adolescent a favorable prognosis may frequently be given, where incision and drainage would at least be followed by uncertain results. But the *sine qua non* is efficient and prolonged mechanical protection. Unless this can be carried out with almost the same care the surgeon gives to his aseptic dressings the result will be doubtful.

Indeed, the value of mechanical treatment can not be overestimated. It is absolutely essential to success. It does not answer to simply apply a support and then leave the patient to the care of the instrument-maker. The surgeon must feel the same responsibility regarding the attention to details that he does in treating a fracture or in following up the dressings after a major operation. He must know that the mechanical treatment is doing the work required of it and that real support is being secured.

The method also is important. For example, recumbency in Pott's disease, except as a temporary expedient, is a fallacy; so is the weight and pulley in hip-joint disease. The various portable frames and beds, the leather and felt jackets, do not afford proper protection to the diseased conditions under consideration. The fundamental principle of mechanical treatment in chronic tuberculous

joint disease is: ***Protection to the diseased part, with the maintenance of functional activity of the other parts of the body.*** The plaster of Paris jacket is not adapted to the treatment of Pott's disease, and I should no more think of applying it to the treatment of this condition than I should of opening an abscess without aseptic precautions. Long ago, in 1878, in my brochure on Pott's disease, I called attention to the fact that the gypsum splint was useless in chronic spondilitis above the eighth dorsal vertebra. When the disease is below that point we have a far more accurate means of support than the plaster of Paris jacket, and a well-adjusted ball-and-socket chin piece is so far superior to the "jury mast" as a support above the eighth dorsal vertebra that the latter should be relegated to the eighteenth-century practice where it belongs.

With an accurately fitting antero-posterior steel support, based upon the Taylor principle, supplemented when practicable by a properly applied belt, making pressure upon and limiting motion at the diseased part of the spine, we have as nearly as may be an ideal protection for the diseased spine.

Why should we limit superior thoracic respiration in dorso-lumbar Pott's disease, or why should we interfere with abdominal respiration in cervico-dorsal disease? The ideal apparatus should support the diseased portion of the spinal column and allow, so far as practicable, functional activity at the non-diseased portion. This certainly can not be accomplished by any encasing jacket which attempts, though it does not succeed, satisfactorily splinting any part of the spinal column.

What is abscess? Technically speaking, it is a "collection of pus formed as the result of suppuration."[1] For the purpose of this paper we call a collection of tuberculous material an abscess, though it frequently does not contain

[1] Foster's *Encyclopædic Medical Dictionary.*

any pus at all. If the abscess of tuberculous joint disease was the result of a pyogenic process this paper would probably not have been written, and there probably would be no important difference of opinion regarding the question of the treatment of abscesses among surgeons. But the cold abscess of tuberculous disease presents, both clinically and pathologically, an entirely different aspect from "a collection of pus, the result of suppuration." Clinically it presents a simple fluctuating tumor. There is no apparent heat, and the general temperature is normal, or nearly so. Pathologically it is not a pyogenic abscess. It contains the product of a distinct condition having the bacillus of tuberculosis as its cause. Ogston, Cheyne, Collins Warren, Senn, and latterly Dr. John Dane, of the Children's Hospital in Boston, as well as others, have proved that these abscesses are sometimes absolutely sterile. Why, then, should we be in a hurry to incise the abscess and permit the entrance of micro-organisms?

Ordinarily the tuberculous abscess pursues a benign course. Frequently we are obliged to search for it, especially in dorso-lumbar Pott's disease. The patient may be wholly unaware of its existence. It gives rise to no inconvenience. It is simply a sign of disease, the location of which is remote from the indolent and ofttimes painless expression of it.

So long as we know of the existence of an abscess in chronic joint disease by our sight or by palpation only, we are perfectly justified in letting it alone. There are no indications for active interference. As has been mentioned, it may be sterile. Then, again, it may be absorbed, or we may cure the *cause* of the abscess before the latter becomes at all pronounced, in which case the abscess becomes a local affair and has no special significance.

But even under the most favorable circumstances the abscess may assume a different phase. The simple tuberculous material may undergo a change and a pyogenic ele-

ment is added. The so-called "mixed infection" then occurs. Under these circumstances we may find a slight daily rise of temperature, showing a minor degree of septicæmia, and sooner or later the abscess may increase in size and the process of burrowing may begin.

It is in conditions of this kind that the judgment, experience, and patience of the surgeon will be tested. The temptation is to use the knife at once. My advice is: Do not be in a hurry to do so. Wait for a while. The septicæmia flurry may and probably will soon pass over—if you protect the articulation. And if the infection proves to be a slight one, the general condition remaining good, with approximately good appetite, sleep, and digestion—wait. Get your patient up—in his apparatus. Let the healthy parts of the body move in the air and sunshine. Even if the temperature goes to 101° or even 102°, do not be worried. In children this is not a serious menace. And if you watch and wait, and maintain the joint protection, you will probably find that the condition will gradually subside.

But if, as sometimes happens, the infection is very severe and the symptoms become urgent, there is no question as to the course to be pursued. Make a free incision and use ample and perfect aseptic means to protect the abscess. But if you can maintain a secure protection to the diseased articulation it is better by all means, if the infection is slight, to let the abscess alone. And if it must open, let it open itself.

A temperature of 101° to 102° or more in chronic tuberculous disease tells a far different story from the same temperature in acute pyogenic abscess. I have stood many times at the bedside of a patient with tuberculous disease and abscess, watching the vital signs with apprehension, but waiting for some decided sign to guide me, and as I have waited, Nature has solved the problem by either a spontaneous opening of the abscess or a spontaneous ab-

sorption. And the patient has been a gainer by my waiting.

To those who watch and wait the problem is solved. Those who watch and wait will, I think, reach the conclusion that I have reached long ago—viz., that the ultimate recovery of the patient with a useful joint is better under the plan of non-interference than it is under that procedure which makes no practical distinction between a tuberculous abscess and a pyogenic abscess, but opens either indiscriminately.

One hears a good deal about the burrowing of abscesses, of the "maceration" of healthy parts by the contained matter, etc. But I have learned, under ordinary circumstances in chronic tuberculous abscess, to have no fear of burrowing, and Nature so far encapsulates a chronic abscess that "maceration" exists largely in the imagination of those who fear it. Surely, in the cases I have reported to the American Orthopædic Association there was ample chance for "maceration" after the extensive "burrowing" that existed. In one case (S. W.) the abscess extended from the diaphragm to the lower third of the thigh, and involved the gluteal region as well. Nearly the same condition existed in the other one (J. B.). But, under efficient treatment, both abscesses were absorbed, and there is not a trace of their existence to-day. So it is in many other instances. The slow burrowing of a chronic abscess need not be feared. The burrowing of an acute pyogenic process demands immediate attention.

Looking at the question in the abstract, we may ask: Do asepsis, incision, drainage, irrigation, etc., meet the indications in our present state of knowledge? Does the knife, in making an external wound which communicates with the diseased joint, favor the ultimate cure of the disease? How often can we be sure, short of a radical excision of the joint (and this is impracticable at the hip and the spine), that we can remove the tuberculous material?

Logically speaking, one should go on and excise the joint if one opens a tubercular abscess connected with it; for unless all the tubercular material is removed there is increased danger with a freshly incised wound, and it is impossible to insure perfect sterilization of an open sinus for a prolonged period of time.

These are questions which I have been studying for years. Through all the phases of Listerism, through all the aspects of a moderately active operative work, through the dreary field of non-interference, I have plodded in my desire to seek the truth. I bring these questions before this audience to-night and ask them to aid in the solution of a most important problem.

As a contribution to the subject I submit the results of four years of patient work in the Orthopædic Hospital. I have no other statistics with which to compare them; but I sincerely hope that some of my colleagues who hold different views will give their side of the question in an equally practical form, so that we may see both sides of the question.

From the first day of May, 1892, to the present date, sixty-three patients with hip disease, eighteen patients with Pott's disease, and twelve patients with knee-joint disease (ninety-three in all) have been treated at the New York Orthopædic Hospital.

During this period Dr. P. H. Fitzhugh and Dr. R. A. Hibbs have respectively filled the position of house surgeon, the former serving to the 1st of January, 1894, the latter being the present incumbent. Their careful work in watching the patients under their care, and their thoroughness in keeping the histories, etc., deserve my warmest commendation.

Of these ninety-three patients with joint disease thirty-five have had abscesses. Nineteen of them occurred in the service of Dr. Fitzhugh and sixteen in the service of Dr. Hibbs.

Only thirteen of Dr. Fitzhugh's cases and the same number of Dr. Hibbs's cases come accurately within the scope of this report, which is to demonstrate that, in the great majority of instances, tuberculous abscess in chronic joint disease pursues a benign course under efficient mechanical protection, and that the most excellent results are obtained under the plan of non-interference with the abscess. Hence, it would be fair to report those cases only which have been watched from beginning to ending, and where the treatment has been carefully and continuously carried out. The last six of Dr. Fitzhugh's cases are not by any means representative ones, nor are Cases XXIII, XXXIV, and XXXV of Dr. Hibbs's series; but I feel that it is better to report *all* the abscesses that have occurred in the hospital, so that the hearer may judge for himself and draw his own conclusions.

The treatment pursued in these cases was:

1. Efficient and continuous mechanical protection *(a)* with the long Taylor traction splint in hip-joint disease; *(b)* with the antero-postero spinal support in Pott's disease; and *(c)* with an immobilizing or traction apparatus in knee-joint disease.

2. By constitutional remedies—cod-liver oil, the hypophosphites, malt, iron, etc.

3. By rest in bed during the treatment of the deformity stage of hip-joint disease and recumbency in the acute phases of Pott's disease. The patients, however, were up and about nearly all the time, the hip deformity yielding easily under ordinary circumstances to the hip-splint traction upon an inclined plane.

4. In all cases the abscess was allowed to open spontaneously, no pressure or force being used, aseptic dressings were applied at once just as if an artificial opening had been made. In most instances the simplest external dressings were used. In a few cases irrigation and drainage were employed. In the majority of instances all that was

done was to keep the parts clean externally by the use of peroxide of hydrogen, bichloride-of-mercury solution, carbolic-acid solution, europhene, etc. All temperatures were taken at the rectum. No iodoform was used.

The following are the condensed histories, photographs of the result being presented in all available cases.

Dr. Fitzhugh's cases are as follows:

CASE I.—Tommy L., aged four years; dorso-lumbar Pott's disease; large abscess, involving pelvic cavity and thigh; duration of disease before abscess appeared, eight months; abscess opened spontaneously one month after its recognition; closed four months later. Treatment: Poultice before abscess opened; after, simple external dressings; no irrigation, no washing out. There was no constitutional disturbance and no increase in deformity. Patient discharged cured.

CASE II.—George R., aged twelve years; hip disease; ilio-femoral abscess primarily, afterward involving outer aspect of thigh; abscess very large; duration of disease before abscess appeared, five months; abscess had entirely disappeared one year later; no constitutional disturbance; no local treatment; no subsequent recurrence of the disease; recovery complete with half an inch shortening; joint motions almost normal; has been working in a store over two years.

CASE III.—Ella G., aged six years; double hip-joint disease; abscess large, ilio-femoral, on left side; no abscess on right side; right hip has completely recovered.

The disease on left side was not recognized until the abscess appeared; it opened spontaneously three months later; a small sinus still remains, discharging a few drops daily; no pain. Temperature during active period of abscess and just after its opening, 99° to 101°; simple external dressings; no irrigation or washing out; general condition excellent; left leg a quarter of an inch shorter than right.

CASE IV.—Lewis L., aged two years and a quarter; abscess after excision of hip joint; excision performed in Scranton, Pa., in March, 1889; the abscess appeared at outer aspect of thigh two years after operation; opened

FIG. 1.—Case I.

FIG. 2.—The resulting cicatrices in Case XIII.

FIG. 3.—The resulting cicatrix in Case XVIII.

spontaneously a month after it appeared; closed five months later; simple external dressings; no constitutional disturbance; recovery; flail joint (due to the excision); three inches shortening.

CASE V.—Minnie G., aged five years; hip-joint disease; had two abscesses—one on the anterior aspect of the thigh; the other, in the gluteal region, was quite large; the thigh abscess occurred thirteen months after the first appearance of the disease; the gluteal abscess two years and four months after first appearance of disease. Thigh abscess opened spontaneously nine months after it was first noticed; the gluteal, one month; thigh abscess closed in eight mouths; gluteal in four months. Treatment, simple external dressings. Temperature after gluteal abscess opened for two weeks, 99° to 104°; since then, normal. Present condition: Extension of thigh to a hundred and seventy-five degrees, with ten degrees of motion; no lateral deformity; abscesses have remained closed; shortening, a quarter of an inch.

CASE VI.—Nettie D., aged two years; hip disease; gluteal abscess of about the size of an orange; duration of disease before abscess appeared, three years and one month; abscess has entirely absorbed after existing for one year; general condition most excellent. Present condition: Extension to a hundred and seventy-five degrees; about ten degrees of motion; slight adduction; one inch shortening.

CASE VII.—Eddie P., aged eight years; hip disease; anterior femoral abscess, size of a goose egg; disease existed eighteen months before abscess appeared; opened spontaneously three months later; a small sinus still discharges a drop or two daily. Temperature, 99° to 101° for some months, but there was no interference with appetite, sleep or exercise. Present condition: Extension to a hundred and sixty degrees; ten to fifteen degrees of motion; very slight abduction; shortening, a quarter of an inch.

CASE VIII.—Joseph R., aged eight years; Pott's disease, dorso-lumbar; large psoas abscess, filling one half of pelvic fossa, existed on admission; eighteen months later, abscess had entirely disappeared; no constitutional symptoms; no increase in deformity.

CASE IX.—Elizabeth O., aged seven years; abscess after excision of hip joint; small abscess appearing at line of cicatrix; excision performed at a general hospital in this city; abscess appeared in March, 1893, opened spontaneously one month later; closed in three months; simple external dressings; no elevation of temperature; an inch shortening; walks fairly well with telescoping joint, due to the excision.

CASE X.—Frank O'N., aged seven years; knee-joint disease; large abscess over internal condyle and involving popliteal region; abscess existed at time of admission; increased very rapidly and opened spontaneously four months later; about six weeks after abscess opened patient left the hospital and went to Buffalo. At that time the sinus was in excellent condition, discharging a few drops only every twenty-four hours; no constitutional disturbance; no irrigation; simple external dressings.[1]

CASE XI.—Alice D., aged two years and a half; hip-joint disease; very large anterior femoral abscess, involving also outer aspect of the thigh; disease existed two years before abscess appeared; fifteen months later abscess has entirely disappeared by absorption without the slightest constitutional symptoms; hip was exquisitely sensitive when patient entered hospital; strongly marked tubercular heredity. Present condition: Extension to a hundred and eighty degrees; flexion to a hundred and forty degrees; no lateral deformity; shortening, half an inch.

CASE XII.—George S., aged ten years; hip-joint disease; large gluteo-femoral abscess, which was present when patient entered ward, having been present ten months prior to that date; five months after admission abscess opened spontaneously; had four sinuses, three of which have closed; one is now discharging a few drops daily; nearly healed[2]; no constitutional disturbance; general health excellent; simple external dressings; shortening, half an inch.

CASE XIII.—James S., aged five years; Pott's disease,

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[1]The patient went under the care of Dr. Bernard Bartow of Buffalo, who reports to Dr. Fitzhugh that the sinus has closed.

[2]January 15th, 1896.—All the sinuses have closed.

dorso-lumbar. On admission, iliac abscess, with small sinus discharging. In very bad condition on admission; thigh flexed to ninety degrees; sinus closed two months after admission. Temperature at first, 99° to 102°. This soon disappeared and patient made rapid improvement; simple external dressing; no increase in deformity; patient well.

CASE XIV.—Robert K., aged ten years; Pott's disease, (1) upper dorsal, (2) dorso-lumbar; two distinct foci of disease; large abscess in left iliac fossa; abscess increased in size for a time; general condition very much improved; no constitutional disturbance; no increase in deformity; patient removed by parents after a nine months' residence in hospital in October, 1893; was last seen May 23d, 1894. Prior to that date abscess had been aspirated, and it subsequently opened spontaneously in February, 1894. A small sinus was discharging when last seen. Patient was in good condition and going to school.

CASE XV.—Yvonne O., aged five years; Pott's disease, dorso-lumbar; large abscess in right iliac fossa; opened spontaneously twelve months after admission; very profuse discharge; irrigation, etc., for several weeks, during which temperature ranged from 99° to 103°. Afterward simple external dressings were used. After being in the hospital for a year was removed by her mother (July, 1892) to the country, the sinus still discharging; spinal deformity slightly increased. She also had a fluctuating tumor involving the upper third of thigh (left). Patient has not been examined since last March (1892), when it was reported that she was in good condition, able to go about, with several sinuses discharging.

CASE XVI.—Nellie C., aged ten years; hip disease; small femoral abscess, which opened spontaneously two months after admission. Irrigation and antiseptic dressings. Temperature before abscess opened, 100° to 101°; after opening, 100° to 104°. A second abscess appeared about a year later on inside of thigh, which opened in a month. Irrigation, etc., for a short time, afterwards simple external dressings.

The patient was doing well and improving when she was removed in the summer of 1892 to a general hospital, and after a brief stay she was transferred to another gen-

eral hospital. She then had five discharging sinuses. She died of pulmonary tuberculosis some time in the winter of 1893.

During her residence in the two general hospitals referred to the patient had little or no protection to the diseased joint. She left the Orthopædic Hospital in a fair condition, walking about on her traction splint. The loss of efficient mechanical treatment was, I believe, an important factor in her subsequent career.

CASE XVII.—Morris M., aged thirteen years; Pott's disease, dorso-lumbar. On admission to the hospital in April, 1893, had four discharging sinuses, with marked double psoas contraction. General condition on admission very poor. Improved greatly in general health for a time. Sinuses dressed externally, with occasional irrigation; no marked elevation of temperature. Was discharged from hospital as incurable, after nineteen months, in November, 1894, with ten discharging sinuses. Died in June, 1895, eighteen months later, with waxy liver and general tuberculosis.

CASE XVIII.—Mary C., aged ten years; hip disease; admitted to hospital, May, 1893, with small sinus on external aspect of middle third of thigh, which closed one week afterward and has remained closed; no elevation of temperature; simple external dressings; joint can be fully extended, and has about ten degrees of motion; no lateral deformity; general condition excellent; shortening, two inches.

CASE XIX.—Maggie G., aged four years; Pott's disease, dorso-lumbar. When admitted to the hospital, in June, 1892, was in a deplorable condition. She had pulmonary tuberculosis, and three sinuses were discharging profusely—one just above the kyphos, the second at the inner aspect of the thigh, and the third just below the trochanter major. Temperature, 101° to 104°. The patient had been treated prior to admission in a general hospital for two years with a plaster of Paris jacket, the one we removed weighing almost as much as the emaciated patient. The condition was hopeless from the first, and after two months' effort the patient was discharged. She died shortly afterward of phthisis pulmonalis.

FIG. 4.—The resulting cicatrix in Case XX.

FIG. 5.—The resulting cicatrix in Case XXII.

FIG. 6.—The resulting cicatrices in Case XXIV.

The following cases occurred in the service of Dr. R. A. Hibbs:

CASE XX.—Arthur K., aged seven years; hip disease; large abscess on outer aspect of the thigh; duration of disease before abscess appeared, five years; abscess opened spontaneously eight months after its appearance; closed three months later; has been closed one year. Treatment, simple external dressings. Temperature, 99° to 100°. Present condition: Extension of thigh to one hundred and eighty degrees; twenty-five degrees of motion; no lateral deformity; general condition excellent.

CASE XXI.—Jennie G., aged seven years; Pott's disease, dorso-lumbar; abscess in either iliac fossa; very large on right side; somewhat smaller on left; abscess appeared about five years after the disease was first noticed. The right abscess disappeared by absorption about eighteen months after it was first noticed; the left also disappeared in about one year. Temperature, 98.4° to 100°. General condition excellent; deformity has not increased.

CASE XXII.—James E., aged thirteen years; hip-joint disease. Previous to admission patient had some extensive operation, probably exploratory, at antero-lateral aspect of thigh, the cicatrix presenting a sinus on admission which discharged slightly for a short time. Subsequently, about one year later, an abscess appeared in same location. It gradually became quite large, and opened spontaneously one month after it was noticed. In four months it had healed; no constitutional disturbance; simple external dressings; extension of thigh to a hundred and eighty degrees; flexion to ninety degrees; no lateral deformity; shortening, an inch and a half; general condition excellent.

CASE XXIII.—Sophie C., aged four years; hip-joint disease; very large gluteal abscess. Disease existed three years before abscess appeared—opened spontaneously ten months later. While under treatment patient developed knee-joint disease on the same side. The gluteal abscess interfered with the perineal pads of the hip splint, and the weight and pulley were used. It was impossible to apply efficient mechanical treatment; abscess continued to discharge, and one day the mother arbitrarily removed patient

from the hospital, after which treatment was refused. Temperature rose to 104° after abscess opened for a short time, after which it became about normal. Shortening, one inch. Patient left hospital with considerable flexion and adduction.

This case is especially interesting as showing how the abscess itself may interfere with efficient mechanical treatment.

CASE XXIV.—Elmer G., aged eight years and a half; hip-joint disease; anterior femoral abscess of an average size. Abscess was present when patient entered ward. It opened spontaneously soon after we first saw it; closed in twenty-one months. Temperature immediately after opening, 103° to 104°; for a few days after that, 100° to 101°; simple dressings only; abscess reopened seven months after it closed; slight discharge for three months, giving no inconvenience. Present condition: Extension of thigh to a hundred and seventy degrees, twenty degrees of flexion; no lateral deformity; general condition excellent throughout; one inch shortening.

CASE XXV.—Mary Z., aged thirteen years; hip disease; extensive abscess on antero-lateral aspect of thigh. Duration of disease before abscess was noticed, seven months; opened spontaneously four months later; entirely closed in eighteen months; simple external dressings; temperature, 99° to 100°; patient showed steady improvement while under treatment; is now in excellent condition; thigh extension, a hundred and eighty degrees; twenty degrees of motion; no lateral deformity; shortening, an inch and a quarter.

CASE XXVI.—Julia H., aged eight years; hip disease, large abscess on outer aspect of thigh. Disease existed twenty-two months before abscess appeared. Opened spontaneously five months later; closed in six months; temperature, 99° to 100°, with occasional rises to 102°; simple external dressings. General condition kept improving during treatment. Present condition: Extension of thigh to a hundred and eighty degrees; twenty-five degrees of motion. No lateral deformity; shortening, a quarter of an inch.

CASE XXVII.—Ada M., aged eleven years; Pott's disease, dorso-lumbar; large ilio-femoral abscess on either

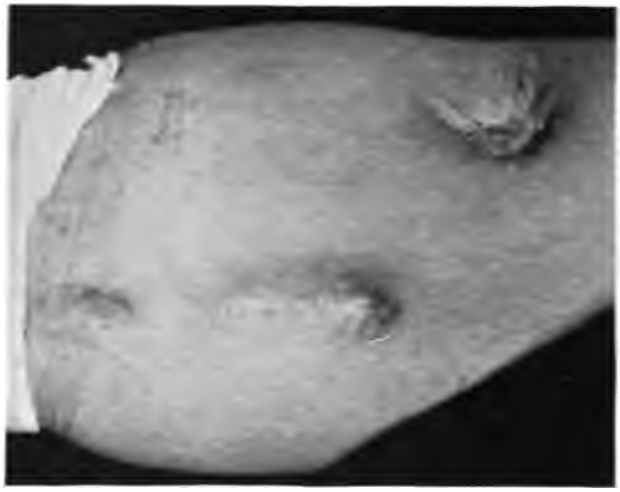

FIG. 7.—The resulting cicatrices in Case XXV.

FIG. 8.—The resulting cicatrix in Case XXVI.

FIG. 9.—The resulting cicatrix in Case XXVII.

FIG. 10.—The resulting cicatrix in Case XXVIII.

side. On the right side abscess extended from diaphragm to middle of thigh, both anteriorly and posteriorly. Aspirated twice without diminution of the pus; a slight discharge appeared at the point of aspiration for a short time. This closed leaving a scar with the abscess as large as ever. The whole mass underwent absorption. No evidence of abscess exists to-day. On the left side the abscess was also very large, but did not extend very far below Poupart's ligament. This abscess has disappeared by absorption entirely in about three years. Neither of these abscesses gave the patient any constitutional symptoms. At one time her temperature rose slightly at irregular periods, and, as she came from a malarial region in New Jersey, Dr. Andrew H. Smith thought it was due to this cause. She was up and about all the time in her apparatus. No increase in the deformity. Patient in excellent condition.

Case XXVIII.—Martha H., aged five years; hip disease; large abscess on outer aspect of thigh. Disease existed a year and four months before abscess appeared. Opened spontaneously four months later. Closed in eleven months. Temperature, 99° to 100°, exceptionally to 101°. Simple external dressings. General condition excellent at all times. Extension of thigh to one hundred and eighty degrees; twenty degrees of motion. No lateral deformity. Shortening, a quarter of an inch.

Case XXIX.—Joe B., aged nine years; hip disease; large abscess on lateral aspect of thigh, which existed when patient entered ward. Opened spontaneously six months later. Closed in ten months. Temperature, 99° to 101°. For four months after opening, irrigation with bichloride of mercury or peroxide of hydrogen was used, but the discharge continuing very profuse, this was stopped. After irrigation was stopped and simple external dressings were used, the temperature fell from half a degree to a degree and a fifth every day, and discharge was much diminished. General condition excellent. Extension of thigh to one hundred and seventy degrees; twenty degrees of motion. No lateral deformity. Shortening, an inch and an eighth.

Case XXX.—Samuel L., aged twelve years; hip disease; very large abscess on anterior aspect of thigh,

extending down to three inches above the knee joint. The disease had existed two years and three months before abscess appeared. Abscess opened five months after it was noticed. It is now closed, eleven months after it opened. One or two crusts appear on the long cicatrix. Immediately after the spontaneous opening the temperature rose to 104°, and remained about that point for a few days. For four months the large cavity was irrigated twice daily, but the temperature remaining high, about 102° to 103°, and the discharge continuing profuse, irrigation, etc., was stopped and simple external dressings were employed. The temperature at once dropped from one degree to one degree and a half, and the discharge diminished. Patient is now in good general condition. Extension of thigh to one hundred and seventy degrees; fifteen degrees of motion. Very slight lateral deformity. No shortening. A second abscess (gluteal) has recently developed. It has opened spontaneously during the past few days.

CASE XXXI.—Abe L., five years old; hip disease; abscess discharging profusely when patient entered hospital. His general condition was very poor. Under simple external dressings his general condition improved very much, and in a year the sinus had entirely closed. Temperature, 98.4° to 100°, with occasional rises to 102°. Present condition: Extension of thigh to one hundred and seventy-five degrees; motion slight—about five degrees; slight abduction; shortening, half an inch.

CASE XXXII.—Lulu R., aged seven years; hip-joint disease; abscess discharging when patient entered hospital. Very slight discharge for a year. Sinus closed. General condition excellent. Temperature, 99° to 100°. Present condition: Extension of thigh to one hundred and seventy degrees; ten degrees of motion; slight abduction; an inch shortening.

CASE XXXIII.—Samuel K., aged five years; knee-joint disease; small abscess over internal condyle. Disease existed ten months before abscess opened. Abscess opened spontaneously seven months later; still discharging slightly.[1] A second abscess appeared on the opposite

[1]Sinus has completely closed, January 1st, 1896.

FIG. 11.—The resulting cicatrices in Case XXXI.

FIG. 12.—The resulting cicatrix in Case XXXII.

FIG. 13.—The resulting cicatrix in Case XXXIII (outer side of the knee). The sinus on the inner aspect is closed, but no photograph is obtainable in time for this publication.

side six months ago; opened two months later; closed a few weeks ago. When patient came under treatment the joint was very flail-like. Patient has improved very markedly. Flail-like motion has become much modified. General condition excellent. Temperature, 99° to 100°. Knee joint straight. Motion through eight or ten degrees. No subluxation.

Case XXXIV.—George K., aged twelve years; multiple joint disease. Patient entered hospital in June, 1893, from an almshouse in St. Lawrence county, New York, where he had been for several years. On admission he had disease in both hip joints, left knee, left shoulder, and right elbow. Also extensive necrosis of left tibia. He had in all thirty cicatrices upon admission.

Dr. McBurney, in consultation, advised amputation at left thigh, and the operation was performed by Dr. McBurney at the Roosevelt Hospital. The history is a long one, and to summarize I may say that the disease in the left hip recovered some months after amputation, and he was fitted with an artificial leg. The patient walked well with the hip splint on the right leg and crutches. Disease in right hip recovered. The sinus at the right knee closed. Patient could walk without crutches and with artificial leg. He suddenly developed albuminuria and died of chronic nephritis on September 28th, 1895. Post-mortem showed recovery at right hip, with an abscess posterior to that joint, connecting with a diseased bone near the ischiadic notch; other joints were not examined.

Case XXXV.—Annie Z., aged eight years; hip-joint disease and lumbar Pott's disease; disease existed three years before abscess, connected with the lumber disease, opened; it opened seven months after its appearance and discharged profusely; patient developed a cough and Dr. Chappell, the attending physician to the hospital, and Dr. Andrew H. Smith, consulting physician, diagnosticated pulmonary tuberculosis; temperature before abscess opened, 99° to 102°; after opening, 100° to 104°; irrigation and injections were used, but the patient failed rapidly and died of phthisis in February, 1895.

Summary.—Of the thirty-five patients (all the abscess cases which have occurred in the hospital for over four

years), twenty-six remained under the care of the institution for a sufficient length of time to test the value of the plan of non-interference.

Of these twenty-six patients, three had each two distinct abscesses, making twenty-nine abscesses treated in all. In two of the double-abscess cases there were large bilateral ilio-psoas abscesses, and it is worthy of note, especially in connection with the cases of S. W. and J. B., that absorption of the abscesses occurred in all these cases.

Of the twenty-nine abscesses, eight (27.58 per cent.) underwent complete absorption; nineteen (65.51 per cent.), after opening spontaneously, closed under simple external dressings in periods ranging from two to twenty-one months; and in two (6.89 per cent.) there are still small sinuses discharging a few drops daily.

Of the twenty-nine abscesses, 93.09 per cent. have either closed or been absorbed.

Of the remaining nine patients, one was removed by her mother after our efforts, up to the time of removal, had failed to produce an adequate joint protection on account of the location of the abscess. In one instance the abscess was nearly well when the patient entered the wards.

In seven instances the patients either entered the wards with phthisis pulmonalis, or had multiple joint disease, or were removed from the care of the hospital while under active treatment. Of these seven, five died and two have small sinuses which discharge slightly.

In conclusion, I desire to say two things: 1. None of these patients were selected, and none were declined, on account of their condition at the time of the application. 2. I hope that others who hold different views, and especially those who practise incision, etc., will make reports of entire unselected groups of patients with tuberculous abscess of the joints. We shall then have a basis for intelligent comparison.

DISCUSSION.

DR. V. P. GIBNEY.—I am quite sure you will all agree with me that this has been a very instructive paper, especially as Dr. Shaffer has taken the pains to follow for four years one line of treatment, and thereby subjected himself to considerable criticism. No matter how much we may differ with him, we must admire him for the courage of his convictions.

Regarding the results obtained by the author of the paper there are certain points to which I desire to refer. In an institution like the one which he represents, while the patients treated are not specially selected, there are certain conditions of admission which make it impossible to receive the very severe cases, such as are apt to drift into a general hospital. Then, again, there are patients who come to the out-patient department at irregular intervals, and are sadly neglected at home; we must take our patients just as they come, and it is hardly fair to compare the results obtained in such cases with those included in Dr. Shaffer's report. When we come to analyze cases in private practice, our results, I think, are better than those obtained in hospital. The impression, I am sure, prevails that it is not well to keep tuberculous patients in the hospital for long periods.

While Dr. Shaffer was reading his report in detail, it occurred to me that in one instance, where he spoke of the difficulty encountered in adjusting a satisfactory protective apparatus in a case of gluteal abscess, it would have been simpler to have rid the unfortunate sufferer of the abscess as well as the dead bone feeding the pus sac, and secured quite as good a result as he did in the other cases of milder type.

I agree with the reader of the paper that there has been a good deal of wrangling on this subject and that the question is still unsettled. From personal experience, I have reached the conclusion that when the orthopœdic surgeon masters aseptic surgery in all its details, he will get results far superior to those narrated in the paper presented this evening. I do not believe in opening a tubercular abscess, scraping it out and then closing it, but I do believe, in certain cases, in laying open the sac from end to end, and in taking sufficient time to get rid of *all* the dead bone, where

this is possible, thus getting rid of the source of the abscess. Of course, I appreciate the difficulty of reaching these foci, but to get better results we must work in that general direction. I am sure we will all profit by Dr. Shaffer's paper and feel more certain than ever before that a certain number of these abscesses will disappear under efficient mechanical treatment.

My opinion regarding tubercular abscesses may be summed up as follows: Where the abscess is small, or even where it is large, and does not interfere with the efficient application of a protective apparatus to the spine or hip or knee, I think that that is a good abscess to let alone. It should be watched carefully, and if found undergoing resolution, then proceed emphatically with expectant treatment. If, on the contrary, we find it getting larger and larger, or, perhaps, burrowing, as it sometimes does, or if we feel that it has become a *pyogenic* abscess, then it is our duty to evacuate, either by aspiration, or that failing, by free incision, making every effort to remove the dead bone which causes the abscess. If it is a psoas abscess, leading up to the dorsal or dorso-lumbar spine, we can often with a long curette reach bits of bone and scrape them away, packing the abscess cavity, in order to have it heal from the bottom. This means a long period of careful, aseptic dressing. If the abscess is so situated as to interfere with efficient mechanical treatment, then it should be removed as soon as possible. The kind that are most difficult to handle are those that have already degenerated into sinuses, with pockets in various directions. It is then very difficult to decide what course to pursue in order to effect a cure. Some are in the habit of curetting from time to time, removing all the tissues possible, but in spite of every care and attention many of these cases go on from year to year, and ultimately the patient develops amyloid changes in the liver and kidney and succumbs readily to the disease. Sometimes these cases recover, in spite of neglect and the worst possible surroundings. So we are still in the dark and I think we must look for relief to the general surgeon, finding out how he does his work, studying the finer details of asepsis, and giving our patients the benefit of it. To such improvements in surgical technique we must add the various mechanical appliances for the protec tion of the diseased joints. This being efficiently done, we

should get our patients out of the hospital as soon as possible, and build them up by means of nutritious diet and out of door life in a favorable climate.

DR. A. B. JUDSON.—I have arranged a few propositions which I will present briefly as my contribution to the discussion. Opening an abscess of chronic tubercular disease of the joint is equivalent to treating a symptom and not the disease. It is dealing with an unimportant incident of the disease under treatment. It distracts attention from the seat of the disease in the bone, where attention is most urgently needed. It is the useless application of a minor surgical procedure in the course of a surgical case of the greatest importance. It subjects the patient to the distress and inconvenience of a prolonged discharge, which would be readily submitted to if recovery depended upon it, but which, if these other propositions are correct, is indefensible. It exposes the patient to the dangers of an infected wound.

So far as my experience goes, it is in accord with these propositions. Beginning studies in this direction twenty years ago, I followed the authorities of that day, and the traditions of the Naval Medical Service, and made early and free incisions. But I saw reasons to change, and for several years have not seen and do not expect to see abscesses of this kind that require opening or any local treatment beyond a simple dressing. This rule of practice has led to some gratifying observations,—for instance, the not infrequent absorption or disappearance of fluid collections coincident with recovery from the bone disease; the spontaneous painless and bloodless opening and collapse of abscesses, followed by rapid and permanent closure, with recovery from the bone disease, and in other cases, in which wide tracts of the skeleton were deeply diseased, multiple abscesses pursuing a long course without serious reaction or impairment of general health, and followed by recovery delayed, not by the deportment of the soft parts, but by the obstinate nature of the disease in the bony tissue.

These statements would be misleading if I did not also recall attention to the necessity of urgent and assiduous mechanical treatment of the disease. I do not mean that the bistoury is to be thrown away, but that its use in these cases should be decidedly the exception and not the rule.

DR. S. KETCH.—I had hoped to hear from some general surgeon on this subject before adding my testimony regarding certain points that have been brought out by Dr. Shaffer in his paper. As he has very well said, the mechanical treatment of tubercular disease of the joints is of the greatest importance in relation to the occurrence of abscesses. and I am quite sure that all orthopœdic surgeons will agree with me that since they have followed the improved methods of mechanical treatment, abscesses are of much less frequent occurrence than they were formerly, and that this question is not such a bugbear as it was twenty years ago.

Dr. Shaffer called attention to the question of abscesses without symptoms, and I am sure we all agree with him that such cases are best treated by leaving them alone. I do not wish to touch upon the nature of these tubercular abscesses, because I believe we have one present to-night (Dr. Dane) who is much more conversant with that side of the question. About eighteen years ago, when I was house surgeon in the New York Orthopœdic Hospital and Dispensary, we had on our visiting staff a number of general surgeons and were quite enthusiastic about the surgical treatment of these cases by the Lister method which was pursued to its fullest possible extent. Every abscess of this nature was incised and then treated with great care, and subsequently all dressings done under the spray. The cases were closely followed for a long time, but in spite of all this the sinuses persisted and the discharge continued, about the same as it does when the abscesses are allowed to open spontaneously. There is another class of cases that I have followed very closely, namely, those in which so-called exsection was performed, and which have come to us for the treatment of abscesses and sinuses following this exsection; of all the abscesses we have had to deal with, these gave us the most trouble and were the most protracted.

I want to say a word in response to the statement made by Dr. Gibney that the cases detailed by Dr. Shaffer were treated in the hospital, and were more or less selected. In the orthopœdic dispensary with which I am connected, where we take the cases just as they come, we pursue the same general line of treatment as we do in the hospital,

and in the same proportion of cases was there a spontaneous disappearance of the abscesses.

I fully agree with the reader of the paper that the so-called cold abscesses which produce no constitutional symptoms should be left alone, because they often disappear spontaneously; on the other hand abscesses of an acute or sub-acute character, which give rise to local and constitutional symptoms, showing infection should be opened as soon as possible.

DR. J. DANE, of Boston, Mass.—I regret that Dr. Bradford was unable to attend this meeting; if he were present, he would be among the first to compliment the author of the paper for carrying out his experiment so fearlessly and for the absolute candor with which he has stated his results. The point that has interested me most in connection with this subject is the nature of these tubercular abscesses. As Dr. Shaffer has pointed out, some increase in size and others do not. It is possible that the increase in size is due to a mixed infection, and for the purpose of determining this I made certain experiments about a year ago at the Children's Hospital in Boston. The experiments were conducted on the theory that there is a variation in the number of leucocytes in the blood when infection occurs. There is a diminution in the number of leucocytes, as compared with the red cells, after infection. I am unable to give you any figures, as my article on the subject is about to be published, and I do not wish to anticipate it, but in all cases where there was mixed infection of the abscess, it showed itself very early by the count of the leucocytes. The latter showed quite accurately what was going on in the abscess. It seemed almost as though we could tell when the abscess became infected. I would be glad to know if any gentleman present has done anything in this particular line of work.

DR. J. A. WYETH.—Modern pathology holds that a tuberculous abscess does not contain true pus. While in gross appearance it resembles pus, the microscope shows that the normal elements of pus do not exist. Under such a condition the danger of general infection from the bacillus of tuberculosis is very small. This bacillus produces a sub-acute inflammatory process, which results in a rich granulation tissue, the base of which is composed of newly

formed cells, the embryonic tissue of the inflammatory process, which hedges in the tuberculous focus and prevents systemic invasion. The tendency of these products of the tubercular process is to undergo rapid degeneration, due in part to the toxic product of the bacillus of tuberculosis (chemical action) as well as by anæmia, partly local and partly systemic. As a result of this retrograde metamorphosis, caseation and liquefaction of caseous material occurs, the product being a white liquid of varying consistence, resembling but not being pus.

Under favorable conditions these collections of tuberculous fluid tend to absorption. In the majority of instances the wall of embryonic cells offers sufficient resistance to invasion of the tuberculous germs into the general system. The liquid is absorbed ultimately and carried away as a harmless product, and the remaining caseous matter undergoes granular metamorphosis and of itself ultimately disappears. Such pathological processes do not have symptoms in any way in common with abscesses proper, which are the seat of acute inflammation caused by pyogenic germs. Pain is not a marked symptom, since they can exist for months, as in psoas abscesses, and are *not* suspected until the collection of this milky fluid is sufficient to attract attention by its pressure upon the abdominal viscera or protrusion due to its bulk.

It is a recognized fact that these tuberculous foci can become infected with pyogenic micro-organisms without a direct or external wound. Certainly if the medulla of bone can become infected without a direct or external communication, it is just as easy to infect a deep-seated tuberculous fluid when conditions for infection are favorable, the germs travelling through the blood and being deposited in a suitable pabulum.

Accepting as I do the foregoing, the treatment of the two classes is clearly indicated. The simple tuberculous focus, under ordinary conditions, may be left alone to undergo absorption, taking pains by careful nourishment and hygienic precautions to increase the normal resistance of the tissues and prevent general infection. On the other hand, when a tuberculous accumulation is the seat of a mixed infection, as determined by the ordinary symptoms of septic infection, local and general, then a careful

aseptic invasion and evacuation of these abscesses is indicated.

Dr. A. M. Phelps.—In regard to abscesses, the object of this discussion is to bring before the profession the latest ideas in regard to the etiology of abscesses, and how the orthopædic surgeon should treat them. In considering the pathology of abscess, what I have to say is taught in many schools of medicine. Dr. Shaffer presents to us a series of selected cases of abscess of joints of a mild type, which have been treated upon the "wait and do nothing" principle, in the 59th street Dispensary. We all know that he is consulting orthopædic surgeon to St. Luke's Hospital. We all know that hospital is and has been used as an overflow hospital to the 59th street Dispensary. We also know, from the report of that hospital, how many abscess cases are under the care of Dr. Shaffer and Dr. Meyer, the visiting surgeon. We also know something of the results in the abscess cases treated there by this do-nothing plan, and we all feel that a report made by Drs. Shaffer and Meyer upon these cases would be very interesting, and that it would go far towards settling this question of the treatment of abscesses communicating with joints.

But Dr. Shaffer presents here to-night only the few cases treated in the hospital of the 59th street Dispensary, and asks us to leave abscesses alone because his results in this series of thirty-five cases are good. He tells us nothing of the series of abscesses treated in St. Luke's and in the out-patient department of the 59th street Dispensary, from which he selected the thirty-five cases reported upon. He only reports upon the thirty-five cases selected during "four years". This report can carry no weight with the profession, as a convincing proof, that it is better to allow a child to carry around a sack of pus or tuberculous material than to at once evacuate it and establish a drainage from the point of disease to the surface of the body, and thereby save the kidneys the trouble of eliminating an absorbed septic product, with all the dangers attending it.

Dr. Shaffer admits in his paper that purulent abscesses should be at once evacuated. It is now established that ninety per cent. of the so-called tuberculous abscesses contain pyogenic germs; in other words, they are puru-

lent and tuberculous, due to mixed infection. This being the fact, Dr. Shaffer will find no fault with the profession if they operate upon and drain ninety per cent. of such abscesses, and for my own part, I will urge the profession to drain the other ten per cent. I have also opened and drained every abscess, tuberculous or purulent, that has come under my care during the past fifteen years, and I have observed none of the awful horrors of such practice pictured by Dr. Shaffer in his paper. But, on the other hand, the hundreds of abscesses which have come to my clinic at the Post-graduate Hospital, treated by this do-nothing plan in various orthopædic institutions with all the awful ravages, from constitutional infection, bone destruction, maceration, infection, and pus burrowing convince me that immediate interference in all cases is the safest and best plan.

The treatment of abscesses should be based upon etiology and pathology and not upon a report of thirty-five selected cases.

The treatment of appendicitis is so based and it is now classified as a surgical disease. If its treatment were based upon the report of the per cent. of selected cases recovering without operation, the mortality would remain as it was during our days of ignorance. Happily, etiology and pathology came to the rescue, and it has come to stay in the treatment of abscess of chronic joint disease.

Abscesses cannot occur excepting they are preceded by inflammatory action. A lesion must occur first, and then the reparative process, which is, a wandering or proliferation of cells; building up of new inflammatory material constitutes this area of normal inflammation. This normal inflammation is produced by trauma, by embolism, by anything which will injure tissue, anything which will excite the reparative process, the process being inflammation. At this time there may be floating in the circulation the germs of suppuration, the streptococci, the staphylococci, etc., or the germs of tuberculosis, which, coming in contact with embryonic tissue, cause infection. If the process of repair goes on uninterrupted, tissues are restored, but the very moment infection occurs, a new barrier of inflammatory material is thrown up around it, and this cavity may be filled with tuberculous material. Subsequent inoculation may take place by the introduction of pyogenic germs, and

then we have a tuberculous and a purulent process together due to mixed infection. This material, lying in contact with tissues, destroys them, except where a fibrous sac forms, known by the older authors as a pyogenic sac, and then when it does, it builds up a barrier around this mass of pus or tubercular material, and the abscess may become encysted. If burrowing takes place in any direction by the corroding and infecting of tissue, the breaking of abscesses into cavities and burrowing into important organs, all the disastrous results from pus burrowing will be the result. If absorption takes place, and pus or tubercular material is eliminated through the circulation, there is likely to follow disease of the kidney and liver.

These abscesses are treated in a variety of ways, and I will briefly note each of the methods and the reason why we treat them in different ways. If an abscess occurs in the palm of the hand, it is likely to follow the sheaths of tendons, and an incision has to be made in order to evacuate the pus and save the muscles and tendons from sloughing. If it occurs in the wrist joint, it may rupture anteriorly and follow the sheaths of tendons. If an abscess burrows and discharges through the capsule anteriorly in hip-joint disease it ruptures directly into the iliacus internus muscle, finds its way through that muscle into the pelvic cavity, and there produces its destructive changes.

There are five or six ways of treating these abscesses. First, by aspiration. The surgeon has virtually discarded the aspirator in the treatment of abscess. He no longer aspirates for empyema, and I can understand perfectly how aspiration of a psoas abscess might be good practice, but as a rule, bad. Aspiration might be desirable in small children, but as I have previously said, as a rule, in surgery the aspirator has been discarded in the treatment of abscesses. The Germans have a method of injecting them with a solution of glycerine and iodoform, and by means of this treatment some brilliant results have been reported. When I look over the literature on the subject, and examine the cases which have been exhibited from time to time in which this treatment has been resorted to, I am not altogether in favor of the injection of abscesses. Upon the whole, I would not think of injecting a joint, where it was extensively diseased, with iodoform and glycerine.

I would advise, by all means, simple puncturing *and intelligent exploration with the finger.* If the abscess cannot be curetted and all the diseased tissue removed, *it is better not to incise the abscess extensively.* It is better to puncture and insert a drainage tube. *I would lay down the rule that abscesses and sinuses should never be curetted where there is dead bone at the bottom of them, unless all disease can be removed.* In those large abscesses where one cannot remove the disease, do not make a big denuded surface. Puncture, establish drainage, and wash out the abscess cavity with a solution of bichloride of mercury, 1 to 2,000. In cases where the abscess appears externally, and the diseased bone can be removed, make a free incision, curette the cavity, close it up, and primary union of the external wound will result. It would be bad practice to put a scoop into a psoas abscess, because the diseased bone is not removed; it is beyond where we can reach it as a rule. Wash out the cavity as thoroughly as possible, insert a drainage tube up to the point of the disease, carry the drainage tube on the end of a stiff probe up to the diseased vertebræ. (Fig. 1.) Following this line of treatment, I have observed that patients do well. Unless you can remove all of the diseased tissue, do not curette. Puncture, wash, and drain.

If we find when we open an abscess that the joint is extensively diseased, excision, of course, may be necessary. In all abscesses of joints which do not show a tendency to absorption, which seem to be tubercular, or joints in which the capsule is ruptured or the abscess is burrowing, I have freely incised, and I think all these cases should be immediately operated upon for two reasons. In the first place, a small incision is made to introduce the finger, and after introducing it *one can intelligently explore and ascertain* the amount of disease present. If you find the disease is extensive, then excision may be necessary or erasion, or whatever procedure we decide upon after we have entered the cavity. I think in cases of all joint abscesses, as a rule, we had better open them, except possibly in those which cause no disturbance, and do not increase in size, *and I am now operating on all.*

To get a clear idea of the pathological conditions present, I have prepared several pathological specimens, and selected from a large number of photographs, a few to

FIG. 1.

FIG. 2.

FIG. 3.

FIG. 4.

FIG. 5.

FIG. 6.

FIG. 7.

FIG. 8.

illustrate the subject, showing the results of abscess, or of tubercular or purulent infiltration of bone. Fig. 2, head and neck of the humerus. It gives you a good idea of what tubercular material does when it lies in contact with bone. You see that the abscess has partially destroyed the head and neck of the bone. There is disease in the head and neck. Cases of extra-capsular disease produce the same symptoms of hip-joint disease, as though it were located in the capsule itself. Fig. 4 shows extensive destruction of the pelvic bone. This is due to the virulent and infecting properties of pus or tubercular material burrowing along the muscles.

Fig. 3 is a case of knee-joint disease, the destructive process having involved the shaft of the bone. You will notice extensive invasion by burrowing and infection. This was a case of osteomyelitis involving the epiphysial lines, separating the epiphysis from the diaphysis.

In Fig. 4 you will notice that there is extensive destruction of the acetabulum. It is entirely gone. This very interesting case shows the way in which pus enters the pelvic cavity. You notice at the brim of the pelvis the bone is still diseased, the pus following the course of the iliacus internus muscle over the brim of the pelvis and into the pelvic cavity itself.

Fig. 5 shows a tubercular cavity in the head of the femur, and destructive changes in the neck of the bone from infection. Figs. 6 and 2 show fine specimens of extra-capsular disease, *and prove the utter absurdity of trusting abscesses to nature and mechanical treatment.*

Figs. 7 and 8 are of a case of old fracture of the neck, extra-capsular with a normal false joint. The specimen was taken from the dissecting room. Behind the head of the bone was an abscess cavity which had caused a dozen sinuses about the thigh. He had worn a hip-splint for years. A simple operation would have cured him. Fig. 9 represents the ravages of pus and tubercular infection of soft parts. The case had been treated with a long traction splint until the pelvis was filled with pus, his joint destroyed, and the entire inside of the ilium necrotic. A few operations restored him to health.

Figs. 10, 11, and 12 show the result of an excision per-

formed to save life. The case had been treated for some years with a long traction splint. The head, neck, and great trochanter had gone through the acetabulum into the pelvic cavity, and were necrotic. Several sinuses acted as sewers to carry away the fœtid discharge. He not only recovered fully, but has a useful limb, with about three and a half inches of shortening when last examined. A severe injury from a fall brought him to the hospital recently, when I again evacuated a pocket of pus.

The reasons why we operate are: 1. To save years of treatment and suffering. 2. *To intelligently explore.* 3. To get rid of foreign material by drainage. 4. To avoid amyloid disease of the liver and kidney by infection. 5. To prevent burrowing into important structures, such as the peritoneum. I have seen a psoas abscess break into the peritoneum, into the bladder, and into the pelvic cavity. I have seen the quadriceps group of muscles destroyed by the burrowing of pus and tuberculosis material. 6. To prevent maceration infection, which leads to extensive destruction. 7. To remove sequestra, if present. 8. To apply the scientific rules laid down both by the surgeon and the pathologist. 9. To avoid the danger of the tuberculous cavity becoming purulent, which accident occurs in 90 per cent. of such cases. 10. To anticipate a possible excision.

We should operate

1. Always when the capsule of the joint is ruptured.

2. In rapid osteomyelitis, or dual inoculations with joint distension, causing pain.

3. Abscesses appearing at points where they may burrow to other important structures, and to cavities, *e. g.*, anterior rupture of the capsule of the hip-joint; the discharge in such cases takes place into the iliacus internus muscle, and at once burrowing into the pelvic cavity; tuberculous or purulent disease of the wrist-joint; discharging among the flexor or extensor tendons, destroying, in such cases, if left to nature, the muscles of the forearm and the tendons in the palm of the hand. Large abscesses in Pott's disease of the spine, which are likely to rupture into the peritoneal cavity, the bladder, rectum, and pelvis of the kidney. Retro-pharyngeal abscess in cervical Pott's, which should always be opened externally

FIG. 9.

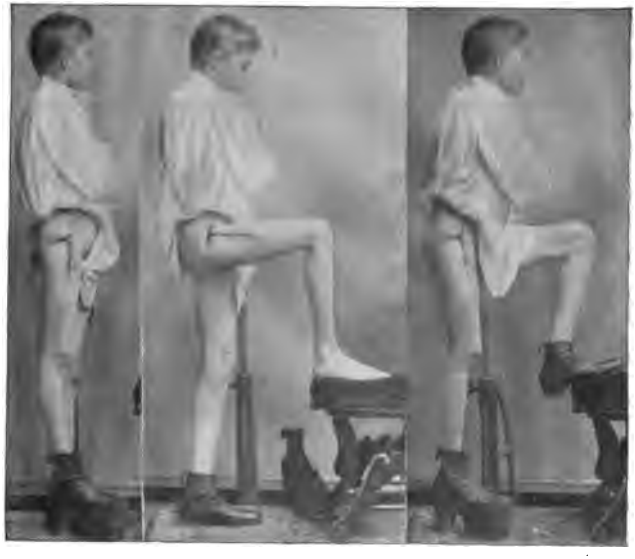

FIG. 10. FIG. 11. FIG. 12.

to prevent infection of the lungs; fluctuating tumors in dorsal Pott's, which are likely to penetrate the pleural cavity or mediastinum; rupture of the capsule of the knee-joint superiorly, the abscess discharging underneath the quadriceps group of muscles; the same is also true when burrowing takes place from any joint. In ankle-joint disease, where the abscess is ruptured posteriorly burrowing upwards behind the tendo-achilles, finding an outlet often three inches above the joint. Such joints cannot drain, owing to the location of the sinus.

4. All tense abscesses, because one can never tell in what direction burrowing will take place.

5. Always when carious or necrosed bone is present. Inasmuch as this can never be determined until the abscess is opened and intelligently explored, I believe that nearly if not all abscesses should be opened and *intelligently explored with the finger, to determine this most important complication;* certainly, in all abscesses where the presence of dead bone might be suspected.

6. Rapidly increasing abscess.

7. Joints filled with tubercular or purulent material. Drainage at the lowest point and the washing out of the joint is certainly a rational, scientific procedure.

8. Repeated occurrence of abscess and multiple sinuses.

9. Abscesses in feeble children, with rise of temperature.

10. In every abscess with rise of temperature.

11. When there is doubt as regards diagnosis. It is safer to explore and drain than to leave the condition to Nature.

12. In every case filled with pus or breaking down of tuberculous material.

In my opinion, it is the best practice where abscess is present in the joint, to follow up the case with appropriate mechanical treatment. When it is impossible to determine the amount of disease that is present, or where the abscess originates, it is better practice to operate on the abscess and intelligently explore it. I have put my finger into a joint and found abscess not only in the joint itself, but external to it. I have found the joint diseased and a focus of disease located in the epiphysis, and frequently the head separated from the neck of the bone.

A case occurred in the Bellevue Hospital, and I secured

the specimen. The man had on him the mark of a perineal strap and other evidences that he had worn a hip-splint of some description. The man lay upon the table with his thigh pepper-boxed with sinuses, and when the joint was finally dissected out, one of the students presented the specimen to me; I made a section of it, and have the photographs of the specimen here (see Figs. 7, 8). This man had suffered from intra-capsular fracture. He was thirty years old. The head of the bone became consolidated in the acetabulum, with the neck of the bone in the new articulation which had been formed by the great trochanter, lying against the neck of the bone. I found a normal articulation, as you will see, between the neck and the great trochanter. I found the head of the bone was consolidated in the acetabulum, and an abscess cavity behind the head in the acetabulum. Cases of this kind should be operated upon and the finger introduced. With the curette, in a few minutes one could cure such a case. The prolongation of treatment would not do any good, and the patient might die of infection, as this one did.

In another case, treated by one of my colleagues for several months, I proceeded at once to follow the sinuses down, and found that the abscess had burrowed along up the radius and ulna, and something like four inches of bone were destroyed. I immediately removed all of the diseased tissue, several inches of bone, shortened some of the tendons, and got a fairly good hand. If this case had been operated on early it would not have been necessary to remove four or five inches of the radius and ulna.

Recent examinations have proven that 90 per cent. of all abscesses which at first might have been tubercular were purulent. In other words, a dual inoculation had taken place. From the fact that 90 per cent. of so-called simple tubercular abscesses sooner or later become purulent, they should be drained early.

Inasmuch as all small tuberculous cavities may at any time become infected with pus microbes and be converted into a virulent, purulent abscess, and inasmuch as we never can tell at what moment such an accident may and quite likely will occur, I claim that we have no moral right to subject a child to such danger. The child with a tuberculous abscess is in danger. It is a powder magazine

that may become infected and explode any day. I see hundreds of them, and every surgeon has verified the same observations, that tuberculous cavities become infected and take on most alarming activity, resulting in extensive destruction of bone and soft parts, all of which might have been avoided by early drainage and care.

As well tell us to watch a case of appendicitis until the abscess had formed and ruptured, as to tell us to let Nature take care of these abscesses. The conditions are similar and the rule of practice governing each should be the same, based upon etiology and pathology.

Tuberculosis produces slow destructive changes: purulent infection a rapid osteomysleti—one produces caries, the other extensive death of bone and soft parts, both giving similar results; only one acts more rapidly than the other. Tuberculosis of joints is analogous to ulceration of soft parts, and Pyogerine infection of bone involving the joint to sloughing and rapid destruction en mass. The principal treatment of each is essentially the same, and it also should be in joint disease.

Dr. R. H. Sayre.—The ground has been pretty thoroughly gone over on both sides, and I do not think I have much to add to the discussion. I would take issue with Dr. Shaffer, however, on one or two points. One is his statement regarding the value of the recumbent position in the treatment of Pott's disease. It has seemed to me that in a certain number of cases I have seen distinct benefit follow recumbency after the patients had failed to improve while going about and taking active exercise. I believe that in some cases the recumbent position is a very essential part of the treatment.

In regard to the treatment of abscesses, Dr. Wyeth has expressed my views about as clearly as I could myself, and I will simply endorse what he said. The question of efficient mechanical support, which Dr. Shaffer touched upon, is perhaps a side issue of the subject under discussion to-night, and in regard to it I would again differ with the statement made by the author of the paper that plaster of Paris is useless in the treatment of Pott's disease.

Dr. L. W. Hubbard.—I agree with Dr. Sayre that the ground has been pretty thoroughly gone over, and there is very little to be added, especially from the

standpoint of an orthopœdic surgeon, whose experience has been largely in the same direction as that of the reader of the paper in the treatment of abscesses connected with chronic articular disease.

I came to my orthopœdic work after service in a general hospital, where we had a large number of abscesses of all kinds, both acute and chronic, some of them connected with diseased joints, and others not. I had been taught during my studies at college and while in the general hospital, that all pus should be evacuated as soon as fluctuation is discovered, and a vast number of the cases which I saw during my service at Bellevue were treated in that way. The acute abscesses all got well after incision; many of the chronic abscesses which involved only muscular structures, also recovered after a certain length of time; of the abscesses connected with the joints, some got well after incision and others did not. I then commenced my service at the N. Y. Orthopœdic Hospital and Dispensary, and at first was inclined to be very incredulous regarding the non-operative treatment there pursued in connection with abscesses of the joints, but as I followed up the cases and saw the results obtained by efficient mechanical treatment alone, my views changed, and I have had no occasion since then to change them again. I am in thorough accord with the statement made by Dr. Wyeth, that some of these abscesses, when they present certain symptoms, should be opened, while others can be safely left alone.

DR. SHAFFER (closing the discussion).—In reply to Dr. Gibney, I wish to state that the cases reported in my paper were not selected ones. They came to the Orthopœdic Dispensary and from there drifted into the hospital. At the Dispensary, we are now preparing a report of the cases of articular abscesses treated there by this same method, which I hope will soon be ready. What we want on this subject is not general remarks, such as those made by Drs. Gibney and Phelps, but an accurate report of every single case coming under their care and treated by their methods, so that we can compare statistics. Let them report their cases, either here or elsewhere, and I will report mine. In some of the cases of which photographs were shown by Dr. Phelps, the extensive maceration of the bone may not have been due to extension of the

abscess; it is well known that abscesses, in extending, follow the course of least resistance. In our present state of knowledge, I think the statement can be safely made, that from eighty to ninety per cent. of cases of tubercular disease of the joints will do better without surgical interference. By leaving such abscesses alone, you will cure the disease in a shorter space of time, and get better results than if you operate.

Stated Meeting, November 7, 1895.

SOME OBSERVATIONS UPON THE EFFECTS OF HORSE-SERUM INJECTIONS.

By HENRY DWIGHT CHAPIN, M. D.

The special appeal that serum therapy makes to all thoughtful physicians is the fact of its not being empiric, but based upon a long and laborious study conducted on scientific principles. At last we seem to have reached a therapy that is directly founded upon a knowledge of the natural history of the disease it is intended to combat. The products of the diseased process in one animal may be employed to check the ravages of the same disease in another animal. Experimenters have not been slow in following up this new field, and protective or curative inoculations of serum have been tried in many diseases. Diphtheria, cholera, hydrophobia, influenza, pneumonia, smallpox, tetanus, tuberculosis, typhoid fever, and syphilis may be mentioned as diseases in which serum-therapy has been proposed, and, in some, given a more or less extensive trial. The serum from various animals has been tried. Thus, Tommasoli treated six syphilitic patients with intra-muscular injections of sheep-blood serum, and the same author used dog-serum upon five cases of lupus. Kollman tried sheep-, calf-, dog-, and rabbit-serum in syphilis, some of the patients remaining under treatment for eight and a half months. In all the studies that have been made, comparatively little attention seems to have been given to the action of blood-serum itself upon the economy, entirely apart from any curative effects upon certain diseases or the development of antitoxins. The new works on serum-therapy do not, as a rule, mention any dangers, or contain any warnings as to

the limit of its use. In the absence of such caution, last winter the writer experimented with horse-serum injections in a number of children suffering from syphilis and allied diseases. The treatment was continued for about two weeks, and all the cases did badly. It then occurred to me that, in serum, we are dealing with a physiological fluid of great potency, and that damage might result from its careless or too prolonged use. A glance at the physiologies shows that serum is described as a transparent, yellowish, alkaline fluid, with a specific gravity of from 1.025 to 1.032, being the liquid part of the blood or of the plasma which remains after the separation of the clot. Serum differs from plasma in that it contains no fibrinogen; it contains paraglobulin and serum-albumin in the same proportion as does the plasma. Thus it appears that these fluids are, above all, highly albuminous solutions, containing nearly as much proteid as is contained in ordinary white of egg.[1]

With reference to the effect of serum, as such, upon other bodies, little is said. Landois and Sterling state that the blood-serum of some mammals dissolves the blood-corpuscles of other mammals. Thus, dog-serum and frog-serum dissolve the blood-corpuscles of the rabbit in a few minutes. They do not say, however, in what manner the serum is applied to the corpuscles. Hermann, under Transfusion, in the "*Handbuch der Physiologie*," also mentions blood-serum as a destroyer of the red blood-corpuscles. Hayem, upon the same topic, states that autopsies on animals that have received transfusion of blood from one species of animal to another, show that of the lesions resulting, those of kidney congestion and changes in the uropoetic apparatus take first rank. In order to study some of the effects of blood-serum injections various animals were subjected to injections for different

[1] Waller's Physiology.

lengths of time and then killed. In all of the cases, plain, sterilized horse-serum was employed. The horses had not been inoculated with any toxins, and were apparently healthy. Certain conditions of the blood were noted before and after the injections.

In 1,000 parts of serum there are:[1]

Water	about	900
Proteids:		
a. Serum-albumin		80
β. Serum-globulin		
γ. Fibrin ferment		
Salts.		
Fats, including fatty acids, cholesterin, lecithin, and some soaps.		20
Grape-sugar in small amount		
Extractives—creatin, creatinin, urea, etc.		
Yellow pigment, which is independent of hæmoglobin		
Gases—small amounts of oxygen, nitrogen, and carbonic acid		
		1,000

July 2d, 1895.—Large rabbit; two months; count of the red blood-corpuscles, 8,316,666; hæmoglobin, 105; 5 c.c. serum injected. In three hours: corpuscles, 7,170,000; hæmoglobin, 90. July 10th, after daily injecting 5 c.c. serum: corpuscles, 8,246,666; hæmoglobin, 95. Autopsy: Spleen enlarged and congested; kidneys showed cloudy swelling.

July 4th.—Sheep, two years old; weight, 60 pounds; healthy; hæmoglobin, 110; 16 c.c. serum injected. In three hours: hæmoglobin, 110; 12 c.c. serum injected daily until July 16th; temperature varied from 104° F. to 105° F. (normal, about 101° F.); respiration and pulse increased, but the animal ate well; no œdema nor infiltration around points of injection. July 16th, temperature 105½° F.; hæmoglobin, 100. The animal was killed by being bled to death. Autopsy: Kidneys enlarged, soft and congested, with all evidences of cloudy swelling; glomeruli prominent, with granular prominences marked over the cortex; very marked congestion at base of pyramids; infarction in one kidney; spleen enlarged.

July 5th.—Guinea-pig; corpuscles, 9,253,333; hæmo-

[1] Kirke's Hand-book of Physiology.

globin, 105; 2 c.c. serum injected. In three hours: corpuscles, 8,453,333; hæmoglobin, 100. More white blood-corpuscles, and quite a number of blood plaques to be seen. July 11th, after daily injecting 2 c.c. serum: corpuscles, 7,783,000; hæmoglobin, 105. Autopsy: Spleen enlarged and congested; kidneys, congestion and cloudy swelling.

July 11th.—Large dog; hæmoglobin, 120; 8 c.c. serum injected. In three hours: hæmoglobin, 118. Daily injections were given, increasing each dose by 2 c.c. until, on July 19th, 24 c.c. were injected, and the animal killed; hæmoglobin, 118. Autopsy: Spleen enlarged, with Malpighian tufts greatly congested; kidneys, capsule adherent; cortex shows cloudy swelling; medullary rays very prominent; pyramids show very marked congestion.

July 18th.—Guinea-pig; corpuscles, 9,320,000; hæmoglobin, 112; 2 c.c. serum injected, and in three hours: corpuscles, 8,970,000; hæmoglobin, 110; 2 c.c. injected daily until July 25th; corpuscles, 10,983,333; hæmoglobin, 106. Autopsy: Spleen enlarged and congested, with Malpighian tufts prominent; kidneys, congested and cloudy swelling.

August 3d.—Large rabbit; corpuscles, 7,436,666; hæmoglobin, 105; 2½ c.c. serum injected, and in three hours: corpuscles, 7,343,333; hæmoglobin, 105; 2½ c.c. injected daily until August 9th; corpuscles, 6,983,333; hæmoglobin, 105. Autopsy: Spleen enlarged and congested; kidneys, cloudy swelling.

In the remaining experiments only three injections were given, in place of the repeated injections of the first experiments. The speedy recuperative powers of the blood are well known, hence the tests were made a few hours after the first injection and repeated at the end of a goodly number of injections in the first experiments.

October 14.—Small rabbit; corpuscles, 8,036,666; hæmoglobin, 98; 2 c.c. of serum were daily injected on October 14th, 15th, and 16th.

October 17th.—Corpuscles, 6,656,666; hæmoglobin, 95. Autopsy: Spleen not enlarged. Microscopical examination of frozen sections of the kidneys showed cloudy swelling and numerous small areas of fatty degeneration.

October 21st.—White rabbit, 1 pound, 5½ ounces in

weight; corpuscles, 11,716,666; hæmoglobin, 85; 1½ c.c. serum injected on October 21st, 22d, and 23d. October 24th, weight, 1 pound, 6 ounces; corpuscles, 10,196,696; hæmoglobin, 85. Autopsy: Microscopical examination of kidneys showed cloudy swelling and a few small areas of fatty degeneration: spleen not enlarged.

October 21st.—White rabbit, weight, 1 pound, 5½ ounces; control, uninjected; corpuscles, 12,040,000; hæmoglobin, 90. Kept under same conditions as former rabbit.

October 24th.—Corpuscles, 11,023,333; hæmoglobin, 85; weight, 1 pound, 6½ ounces. Autopsy: Kidneys normal; microscopical examination.

October 30th.—Rabbit, 1 pound, 15 ounces; corpuscles, 9,686,666; hæmoglobin, 95; 1½ c.c. serum injected on October 30th, 31st, and November 1st. November 2d: Urine had specific gravity, 1.015, with a trace of albumin. Under the microscope, a large number of abnormal blood-corpuscles and a few fine granular cells; corpuscles, 8,083,533; hæmoglobin, 85; weight, 1 pound, 15 ounces. Autopsy: Kidneys showed cloudy swelling and some fatty degeneration.

October 30th.—Rabbit, 1 pound, 7½ ounces; control, uninjected; corpuscles, 9,206,666; hæmoglobin, 105. November 2d: Microscopical examination of the urine showed a number of abnormal corpuscles and one fine granular cast; no albumin; corpuscles, 7,233,333; hæmoglobin, 105. Autopsy: Microscopical examinations showed cloudy swelling of the kidneys and some fatty degeneration.

November 4th.—One per cent. of egg albumin was mixed with a six-tenths per cent. salt solution; 2 c.c. of this was injected into a guinea-pig on November 4th, 5th, and 6th. November 7th the urine contained a faint trace of albumin, a few abnormal blood-corpuscles, and two or three fine granular cells; no casts. Autopsy: Kidney, slight amount of cloudy swelling; liver, marked cloudy swelling and fatty infiltration.

As a control, another guinea-pig was injected on November 4th, 5th, and 6th with 2 c.c. of a simple six-tenths per cent. salt solution. November 7th: Urine, specific

gravity, 1.018, and trace of albumin; a large number of abnormal blood-corpuscles and several fine granular cells. Autopsy: Kidney, simple cloudy swelling; no fat; liver, cloudy swelling and fatty infiltration.

In attempting to draw conclusions from experiments upon animals, especially rabbits and guinea-pigs, two considerations must be borne in mind. The worry and annoyance to which they are subjected by repeated handling and opening of vessels, with a certain loss of blood, has an unfavorable effect upon their health. Again, these animals that have been cooped up for experiments are often in an unsound condition to begin with. Thus, two of my controls had about as bad a state of the kidneys as the animals experimented upon. In Dr. Vissman's experiments with rabbits, however, the injection of antitoxin serum invariably produced nephritis, which was not found in the controls. In summing up the experiments here narrated, they are to a certain extent inconclusive, except that the injections of the serum do not appear to have produced any very marked effect in these animals, save upon the kidneys. Serum injections, as ordinarily employed in therapeutics, probably have little effect upon the blood-corpuscles. The kidneys are the vulnerable point, and must always be carefully watched when injections are employed. Another possible danger is noted in the June number of the *London Veterinary Journal.* It is here stated that M. Nocard recently reported to the Society of Biology of Paris the results of some experiments that he has been conducting, from which it appears that the blood sometimes contains microbes, although no germs could be found by microscopic study of the blood. Cultures made from the serum of blood, collected with such precautions as to prevent the possibility of infection, indicate that microbes are sometimes present, and especially during digestion. The latter has led M. Nocard to believe that microbes find their way into the blood through the intestinal wall.

Examinations made upon dogs recently fed, and upon fasting dogs, have shown that the blood of fasting dogs is always sterile.

In conclusion, the final verdict upon serum injections and serum-therapy must rest upon clinical, rather than laboratory, evidence. Studies in the laboratory thus far made, warn us to use this powerful agent carefully. If prolonged and careful clinical observations made under varying conditions and by different observers prove its utility, serum-therapy, in spite of possible dangers, will have a brilliant future. Research in the line of separating the antitoxins, so that they may be given in an innocuous vehicle, or highly concentrating them in small quantities of serum, seems to be the next step desirable. I am indebted to Dr. Gill for supplying me with serum, and to Drs. Brooks, Wyman, Lewright, and Wilson for aid in the experiments.

SOME PATHOLOGICAL CHANGES PRODUCED BY THE INJECTION OF SERUM.

BY WILLIAM VISSMAN, B. A., M. D.

From what we have learned from the experiments of Landois, Naunyn, Francke, and many others we are not justified in the subcutaneous introduction into the human organism of the blood serum of the lower animals; because it does produce a very serious effect, even on healthy persons. In diphtheria antitoxin, however, we have a modified serum, which its discoverer informs us is almost indifferent in its effect on the human organism.

So far as we can learn from the reports of those who have administered it to man, its effect is far from indifferent. As it is almost exclusively used on patients suffering with diphtheria, and very few deaths have occurred from its use in healthy persons, it is almost impossible to determine the anatomical lesions which it produces, from examination of persons. Particularly since nearly every one of the changes it causes is found in patients suffering from diphtheria.

A few changes have been observed which were brought about by its use in man. Variot[1], Hutinel, Debove, and others found that, if they introduced horse serum (matters not whether the horse had been previously immunized or not), the patient had a rise of from one to three degrees in temperature, acceleration of the circulation, which was often followed by heart weakness and arhythmic pulse.

The exanthema, which is so frequently produced by the introduction of antitoxin, has been observed by Bertin after the use of normal horse serum.

Huebner[2] states that, in the beginning he used a dose of

[1] *Journal de Clinique et de Thérapeutique enfantales*, No. 14, 1895.
[2] *Deutsche Med. Wochenschrift*, No. 42, 1895.

1,110 immunizing units, with 7.5 per cent. of paralyses following, while of late he has used 1,700 units, and 12 per cent. of paralyses followed.

Alfoeldi[1] administered 100 immunizing units, as a prophylactic, to a three-year-old child, which had been exposed to diphtheria. Two days afterward the child had a temperature of 40° C., pain in the lumbar region, albuminuria, hemeturia, and petechia over the whole surface of the body. On the fourth day the child died with not a trace of diphtheria or any other disease.

Heckel[2] found so-called peptonuria in six persons after the administration of antitoxin. Five of these had diphtheria, but the sixth was healthy.

The above-mentioned instances of ill effects of the antitoxin are not absolutely above suspicion that the patient might have been suffering from diphtheria; for nearly all of them were exposed to infection, and the effects noted may be due to this cause, because nearly all of the changes noted have been found in the course of the disease. But at the same time, we must not forget that Landois found about the same changes, which were brought about by the transfusion of blood-serum.

Lowey and Richter[3] found that after the injection of antitoxin into rabbits, quite a change in the blood followed. They introduced 5 c.c. into the circulation, with the following results:

Alkalinity in 100 c.c. blood in mg. of NaOH.	Leucocytes in 1 mm. blood.	
374	7,600	Before injection.
240	4,000	10 minutes after injection.
240	4,000	43 minutes after injection.
309	12,000	24 hours after injection.
330	10,800	48 hours after injection.

Nearly all are agreed that there are certain disagreea-

[1] *Pesther Med.-chirg. Presse*, 1895-XXXI-224.
[2] *Münchner Med. Wochenschrift*, 1895-XLII-159.
[3] *Deutsche Med. Wochenschrift*, 1895-XXI-240.

ble results from the use of antitoxin. Among these complications are erythema, hemorrhages in the skin, heart weakness, pain in the joints (with or without swelling), which may last for several days. A few have observed albuminuria more frequently, others less frequently, while still others consider albuminuria an indication for the administration of more antitoxin. Since albuminuria is rather a frequent complication of diphtheria, it is rather difficult to determine whether it is the result of diphtheria or antitoxin.

Now, as nearly all of the viscera, and especially the kidneys, are more or less affected by infection with diphtheria, it is impossible to determine the action of antitoxin upon them unless we introduce the serum into healthy individuals. For this purpose man is not available. Hence, animals must be resorted to. And, as the experiments published by the author[1] are the only ones, following out this line, that are known to me, it is hoped you will pardon a resumé of the results there reported.

To obtain light upon the effect of antitoxin upon the kidney, it was injected into healthy rabbits.

As the character of the lesions was the same in all the animals, varying in intensity according to the dose administered, it will not be necessary to give a detailed account of the changes observed in each animal.

At the site of injection was a nodule which extended over a radius of about an inch from the point where the needle had entered. The nodule was slightly painful on pressure, but the general condition of the animal was good so far as could be observed. Temperature was not taken. At the end of the second day the induration began to grow smaller, and was scarcely perceptible on the animals killed the seventh day after the injections.

On cutting through the nodules, the subcutaneous tissue was found to be impregnated with a straw-colored serous

[1] *Med. Record*, 1895-XLVIII-374.

fluid containing very few leucocytes. This fluid was always found to be sterile. As time elapsed the œdema grew less marked. As the object of the investigations was to determine the action of antitoxin upon the kidneys, little attention was paid to other organs, more than to note that the spleen and liver were slightly swollen and redder than normal. The kidneys of animals killed twenty-four hours after the injection were very firm and of a dark red color. On the cut surface, the cortical substance was very much injected, slightly opaque, and a little more prominent than the medullary substance, which was pale in color.

On microscopic examination of fresh sections in 0.6 per-cent. salt solution, the malpighian bodies and capillaries of the cortex were found to be distended with blood. The epithelial cells of the tubules were filled with refractive granules obscuring the nuclei, which were only brought to view by the addition of dilute acetic acid. There were no casts found in the tubules. These conditions were found in all of the animals, and, as the time after the administration of the antitoxin increased, the injection of the blood-vessels decreased and the cloudy swelling became more prominent. Those animals which received more than one dose had slight hemorrhages into the tubules. Fat metamorphosis of the epithelium was not observed in a single instance. It is highly probable that had these sections been subjected to fixing by alcohol, or other fixing agents, as was the case with von Kahlden's experiments, the cloudy swelling above described would have been over-looked.

Since the doses administered to the rabbits were about equal to or a trifle less, proportionately, than the doses administered to children in cases of diphtheria, it is reasonable to suppose that approximately the same changes are brought about in children when antitoxin is administered. Certainly, this is assuming that a pound of rabbit

is equivalent to a pound of human subject. Yet, this is no greater assumption than the first use of the remedy was based upon.

Since the injection of serum into rabbits produces a cloudy swelling of the kidneys, or an acute nephritis, as shown above, if all of those persons having virulent diphtheria bacilli in their fauces were treated with antitoxin, many would be injured for an uncertain benefit to a very few. After the disease has made such progress that it can be diagnosed, the toxin has produced such an effect upon the organism that, judging from the experiments of Behring, Parks, and others, little benefit is to be expected from the antitoxin. And since observing the results of experiments on rabbits, just reported, it is highly probable that, instead of reducing the mortality of diphtheria, antitoxin has really increased the death-rate.

PHYSIOLOGICAL ACTION OF SERUM INJECTIONS.

BY WILLIAM HENRY PORTER, M. D.

Two theories have been advanced by which to explain the mode of action of serum injections: the chemical and the so-called vital.

The chemical theory assumes that the serum or some or its contained constituents directly neutralize the toxic agent that is the essential element in the production of the diseased process, thus arresting any further damage to the tissues or cells of the body by the presence of the toxic agent within the system. This theory appears, however, to have been already conclusively disproved; therefore it can be discarded at once as of no practical value.

The so-called vital theory assumes that the serum of some of its contained constituents act through the agency of the living body, thus rendering the cells tolerant of the toxins of disease.

What phenomena are intended to be covered by this vague term "agency of the living body" is not elucidated. What information does it convey in reference to the minute chemical changes which are taking place in the animal economy that are at work antagonizing the toxic compounds and the resulting pathological processes? None, so far as can be determined from the statement as it stands.

This much we do know, however: that so long as the cells of the living body are alive, they are always undergoing certain active chemical changes. These mutations are of three distinct kinds: isomeric transmutation; physical, not synthetic, anabolism; and oxidation, or katabolic destruction. By the continuous action of these three phe-

nomena the cells and all the tissues of the living body are constantly being built up, sustained for a time, and then broken down and eliminated as the katabolic products of oxidation. When these changes are maintained at a certain standard of activity and character, we have the condition described as the normal state of the system. It is the chemical condition, in which the animal economy, taken as a whole, possesses the highest degree of resistance possible against the damaging influences of all kinds of foreign and toxic agents.

When the rate and perfection of the isomeric transmutation and physical anabolism of the molecular elements constituting the cellular structures of the living body are lowered, together with imperfect oxidation and imperfectly formed katabolic products, the chemical resistance of the cell protoplasm against the invasion and damaging effects of all forms of irritants and toxic substances is reduced to the lowest ebb possible.

On the other hand, when there is an increase in the rate and perfection of the isomeric transmutations, physical anabolism, and oxidation of the molecular elements which constitute the cell protoplasm, thus raising its nutritive activity up to or above the normal standard, the chemical resistance of the living cellular structures against all irritants and toxic substances is raised to the highest standard possible. When this condition of the system is reached, the toxic agents cease to produce any abnormal changes in the cellular structures. The toxins are either tolerated by the living cells, or they are drawn into the cell protoplasm, together with active oxygen, and directly oxidized into nontoxic compounds, which are then eliminated from the system by the action of the excretory organs of the body.

With this conception of the chemico-physiology of the animal economy we are placed in a position to study intelligently not alone the effects upon the system of the

plain horse-serum injections, but also that of all kinds of modified serums, and all forms of chemical compounds.

The more familiar the investigator is with the chemical laws which control the animal economy, the more certain he becomes that a definite chemical explanation can be given for the physiological action of every therapeutic agent employed, provided these chemical phenomena are correctly interpreted. It makes no difference whether the compound used is obtained from the mineral, vegetable, or animal kingdom; they are all definite chemical substances which are foreign to the animal economy. They all obey well-defined chemical laws, and act by one of two methods:

First, the substance used is a simple foreign body, more commonly an inorganic compound, although it may be organic in nature, which acts, not by undergoing chemical changes in itself, but rather by virtue of the irritating or poisonous properties that it possesses when in contact with other chemical substances; thus, by its physical presence, stimulating the inactive chemical phenomena of the living cell protoplasm into a condition of increased activity and higher kinetic energy. This physical irritation produced by the foreign body excites increased chemical activity, so that there is an increase in the isomeric transmutation and physical anabolism of the molecular elements that constitute the inherent structures of the protoplasm of the cells. This is followed by a higher degree of oxidation and the production of more perfect katabolic products. This accomplished, it proves that a higher grade of nutrition is instituted, and a much greater tolerance for, or resistance against, irritant and toxic agents of all kinds is firmly established.

It is by the combined results of all these phenomena, or the intrinsic chemical actions of the animal economy, which are so vaguely and frequently spoken of as "the vital action of nature," that the system is enabled to pre-

vent the development of disease, and to eradicate the diseased process and its concomitant symptoms from the body when it has been established.

Second, the substance employed is one which, from its organic nature, derived either from the vegetable or animal domain, is capable of acting, not alone as a simple irritant, but as one that can be rapidly changed isomerically, or taken into the living cells, with active oxygen, there to be oxidized into simple katabolic excretory products; thus yielding heat and exciting active chemical mutations in the living cells, which is of greater intensity and higher kinetic energy than occurs when the normal foodstuffs are introduced into the system. In this manner there are produced the same increased isomeric transmutation and physical anabolism of the molecular structures of the living cells as occur when the purely inorganic compounds are employed in conjunction with the foodstuffs; thereby augmenting oxidation, assimilation, and nutrition in general until a higher tolerance for or resistance against all forms of irritants and toxic agents is developed in the living cells.

It is a well-known fact that the introduction of the toxins of disease into the animal organism causes a decided change in the normal chemical mutations of the cells of the body. When these transmutations are so modified, as previously described, that the toxin is rendered inert, or is split up and eliminated from the system, the cellular elements return to that condition which is termed the normal state. In this so-called normal condition the chemical structures and functions of the living cells of the body are so modified that in the future they have a higher degree of resistance against this or that particular toxin or disease. Or, in other words, the system is rendered in a measure immune to the action of that special toxin, and the disease which it is credited with producing.

Vaccination, or inoculating the human system with the virus of cowpox, causes a similar change in the human organism, so that the vaccinated individual becomes immune to the direct inoculation with the virus of smallpox, or, if infected at all by the smallpox virus, the damaging effects upon the system are of a much milder character than occur in those who have not been protected by the previous influences of the cowpox virus.

These two facts have caused the scientific investigator to try almost every chemical compound known to man, with the hope of discovering some agent that will, when introduced into the system, cause similar changes in the chemical mutations of the living cells, without injurious effects, thereby rendering the cells of the body more tolerent of, or antagonistic to, the toxins of disease. So far this has not been accomplished, although all the inorganic substances, and the well-known chemically formulated compounds obtained from the vegetable kingdom have been faithfully tried.

It is also well known, that by introducing into the system certain inorganic substances and some of the well-defined chemical constituents derived from the vegetable domain, the phenomena which follow the invasion of the animal organism with the toxins of disease can be greatly modified. The chemical mutations of the cellular structures can be so altered that the toxins become, in a measure, inert, or they are more rapidly destroyed and eliminated from the system by the excretory organs. Thus the duration and intensity of the pathological process consequent upon the infection and the concomitant symptoms are decidedly decreased, both as regards their intensity and duration. The discovery of bacteriology, as to the part that the micro-organisms play in the causation and modification of the chemical phenomena that are developed in the protoplasm of the cells of the animal economy in connection with disease, together with the

well-known fact that the action of the toxins of disease upon the chemical processes tends, in a large measure, to immunize the system so affected against a second attack of the same disease; and the undeniable fact of the immunizing power of vaccine virus against smallpox,—have led to repeated attempts to develop out of the animal economy some agent which would have a decided immunizing and curative property.

Whether using the plain blood-serum, or that which has been modified by repeatedly inoculating the animal from which the serum is taken with the isolated toxin of disease, we have no means as yet for determining the exact chemical composition of the fluid injected. With the plain horse-serum the presumption is, that we are injecting a solution of mixed proteids, varying in percentage strength from five to ten of the proteids. Assuming that the animal from which the serum is obtained is in a perfectly normal condition, there is no certainty as to the exact percentage of the contained proteids in the serum injected. When this serum is withdrawn from its natural channel, and is injected into the subcutaneous tissue, or the blood-vessels of another animal, it is introducing a foreign body into the system, just the same as if an inorganic substance, or an organic compound derived from the vegetable kingdom, is administered hypodermatically.

Three questions naturally arise at this point, to wit: Is the blood-serum inert? Is it, like the inorganic compounds that pass through the system, unaltered, acting, if at all, only by its irritating or physical presence? Or is its action due to the contained proteid constituents being drawn into the protoplasm of the living cells, together with active oxygen, there to be oxidized into simpler forms, thus stimulating the epithelial cells into a higher or lower grade of activity?

That serum injections are not inert is fully established

by the physiological experiments that have been instituted, and which prove conclusively that, when large volumes of serum are transferred from the blood-stream of one animal to the blood-stream of another species, there is a rapid destruction of the red blood-corpuscles. Whether this is brought about by the directly irritating properties of the serum upon the blood-cells, or by the serum being a foreign body that must be taken up into the protoplasm of the living epithelial cells constituting the excretory glands, together with active oxygen, there to be oxidized and split into simpler chemical compounds or katabolic bodies that can be excreted from the system, has not been absolutely determined. As the proteids contained in the blood-stream are all in a non-diffusible state, they cannot pass through the system and act as simple irritants only. Consequently, they must be isomerically changed or split into simpler katabolic bodies before they can be eliminated, and thus relieve the system from the irritating influences of the foreign substance. It is therefore highly probable that the chemical properties of the living cells are largely overtaxed in their attempts to destroy and eliminate the injected serums and their contained compounds from the system. This produces a general decrease in the chemical and nutritive activity of the cells and tissues throughout the whole body, with an associated destruction of the red blood-corpuscles and marked leucocytosis, the same as occurs in connection with the introduction and attempt at elimination of all the well-known toxins. This latter view is further substantiated by the results of the experimental work that has recently been done by Drs. Chapin and Vissman, and which has just been presented to the society.

This being the position of serumtherapy, both the plain horse-serum and the modified serums, called antitoxin, must be investigated in the same manner as is applicable to all the inorganic compounds and the chemical substances derived from the vegetable kingdom. They must

be considered as foreign bodies that are inert when injected into the system, that act by their physical presence and irritating properties, or that act by being drawn into the protoplasm of the living epithelial cells, together with active oxygen, where they are oxidized or split up into simpler compounds, which can be easily eliminated from the system by the action of the excretory organs. This much accomplished, it must next be demonstrated conclusively that, while the serum is overtaxing the chemical activity of these living cells to split up and eliminate the plain or the modified serums called antitoxin, the living cells are thereby stimulated and caused to assume at the same time a higher state of nutrition and chemical resistance against other toxic agents. This improvement must be carried to so high a degree that the cells actually become more tolerant of the original toxins of disease, or that they are more capable of oxidizing the toxins and excreting them from the system, so that the ultimate damage to the animal organism is less than occurs when the serums are not used and nature is left alone to accomplish the task by her own unaided powers. This much settled indisputably, it must be then shown that serumtherapy accomplishes these results more successfully and with greater rapidity than any other line of treatment now in the possession of the profession.

While we have no certain knowledge as to the changes that are produced in the blood-serum of the animal by repeatedly inoculating the system with the specific germ or toxin of diphtheria, or of any other disease, until no symptomatic reaction follows the injection, it is quite reasonable to assume that the contained proteids of the serum are somewhat isomerically changed so that they are not absolutely in the same form as they exist in the unpoisoned animal. There may even be a chemical change in the serum. In either case it is quite reasonable to assume that, all things being equal, the change that is produced, if

any does occur, is a fairly uniform transmutation. Therefore the defenders of serumtherapy are justified in claiming they are dealing with a definite quantity; but they must also admit that from a strictly chemical and physiological standard all serums and antitoxins are so far absolutely unknown quantities.

The action, therefore, of all these agents is purely speculative, excepting possibly the diphtheria antitoxin, which does rest upon what is called a pretty sound experimental foundation. By injecting a pure culture of the Klebs-Lœffler bacillus or its toxin into the vaginal wall of a guinea-pig, a true Klebs-Lœffler bacillus membrane is produced. After developing this pure type of diphtheritic membrane in this manner, injections of the diphtheria antitoxin obtained by the use of the Klebs-Lœffler bacillus, or its toxin, cause an arrest of the local process. The injection of this antitoxin into the guinea-pig prior to the inoculation with the Klebs-Lœffler bacillus renders this animal immune.

Before applying these results to the human organism direct, it should be borne in mind that the guinea-pig is naturally immune to diphtheria, while the human subject is especially susceptible to diphtheria. Again, the experimental diphtheria is a pure and simple Klebs-Lœffler form, while that which occurs in the human economy is more likely to be a mixed infection. Consequently these two conditions cannot be compared without making due allowance for these very wide differences.

In the practical application of diphtheria antitoxin, from a clinical standpoint, we find the common statement antitoxin of diphtheria is of avail, only when it is used early and in mild cases of this disease. In the later stages, and where the disease is of a severe type, the antitoxin is of little value, and should not be used; in the diphtheria with mixed infections it has no influence of value. Neither does it render the human subject immune to diphtheria,

but rather it tends to lower the nutritive resistance of the living cells of the body, and thus increases the susceptibility to the disease.

Yet, in the face of all these statements which have been made by the experimental biologists, innumerable statistics are being published which appear to prove conclusively that the mortality from diphtheria has been greatly reduced by the introduction and use of the diphtheria antitoxin. The real facts, however, appear to be as follows: The diagnosis of diphtheria for a long time was based upon the presence of the Klebs-Lœffler bacilli in the throat. The presence of the bacillus, even in the absence of clinical manifestations, constituted a case of true diphtheria. This naturally swelled the total number of the mild cases of diphtheria that would in all probability have recovered upon almost any rational plan of treatment. This single factor has greatly increased the total number of cases, and it alone helped largely to lower the percentage of fatal cases. The severe cases not amenable to the antitoxin treatment, are eliminated, which again helps to lower the apparent mortality. Then a large percentage of cases, having a pseudo-membrane in the throat and all the clinical manifestations common to diphtheria, and in which the mortality runs high in consequence of the mixed infections, but in which the examiners failed to find the Klebs-Lœffler bacillus, have been excluded in compiling the statistics. Of the class some observers put the number as high as 30 or 40 per cent. of all cases of diphtheria. All of the above enumerated classes of cases have helped to keep down the apparent fatality of diphtheria.

If, now, some of the first class, which, from a clinical standpoint, never had true diphtheria, are deducted, as they should be, and the two other classes added, the true and total mortality from diphtheria will be found not to have been perceptibly lowered. On the other hand, there

appears to be an accumulation of evidence which, in the near future, and in the absence of any severe epidemic, will show conclusively that the mortality from diphtheria has been raised rather than lowered by the employment of the diphtheria antitoxin. Until all these differences in opinion, shifting of figures, etc., can be scientifically adjusted, it is absolutely impossible to state accurately just what the true status of serumtherapy is; but the weight of evidence points strongly toward failure along the whole line. Not but that the serums may have a place in the materia medica, the same as many other chemical compounds have that are of comparatively little value. They will assume the position, however, of compounds that may be of value in some instances, but not as sure cures for any specific disease in a large majority of cases in which they are employed.

DISCUSSION.

DR. W. H. PARK.—The evening is hardly long enough to take up the whole subject of antitoxin treatment, and I will confine myself closely to the question of serum and perhaps serum antitoxins in health and not in disease. I will first take up its effects in the experiments on animals. The injection of moderate amounts of serum in animals produces a moderate reduction of the number of red blood cells. We do not find broken down cells, but simply a moderate reduction in their number, which reaches the normal again within one or two weeks. There may be a still further reduction if the injections are rapidly repeated, or large amounts of the serum employed. Upon the leucocytes we find almost no effect from the serum alone, but apparently from the serum antitoxin, we may find a moderate reduction in the number of leucocytes during the first few minutes or hours after the injections are made.

Regarding the influence of the injections upon the kidneys, the following experiments are of interest:—Three rabbits were given to Dr. George P. Biggs for examination; one had received injections of serum, the other of

serum antitoxin, and the third had received no injections whatsoever. He carefully examined them and reported that he could barely discern any difference in the appearance of the kidneys of the three animals, but that perhaps in one of them there were slight evidences of granular change. He then gave the kidneys to a well-known pathologist, telling him that two of the animals from which they had been removed, had received injections, while the third had received none. This pathologist reported that the kidneys taken from the animal which had received no injections whatever showed the most marked changes. I merely state this incident to show how difficult it is to make histological examinations of this character, when the changes in the kidneys are slight.

Fortunately, it is not necessary for us to depend entirely upon the changes found in animals, for in New York city alone, 812 children have been immunized against diphtheria by injections of antitoxin. These children were all healthy, and their ages ranged from a few weeks to twelve years. In a certain number of these the injections produced a rise of temperature; in a few it produced a rash; in about one-tenth the number, albumen was present in the urine for a few days or hours. In none of the cases were any permanent ill effects noticeable. We have therefore no reason to believe that injections of serum and antitoxins in moderate doses, in healthy subjects will cause any serious deleterious effects. It seems to me that these animal experiments themselves show us that the dangers are very slight. At any rate we must depend upon the result of the injections in children and human beings, and if we believe that the antitoxin injections do good in diphtheria and more than counterbalance its evil effects, we should continue to use it.

We will try and get rid of the serum and employ the antitoxin in an easier and perhaps somewhat safer method.

DR. J. S. BILLINGS, JR.—During the past summer I have made certain experiments in the laboratory of the New York Board of Health, to ascertain what effects antitoxin produces on the blood of healthy individuals and also on the blood of diphtheria patients. As control experiments I have examined the blood of five or

six diphtheria patients who had received no injections of antitoxins.

In the fifteen cases of normal individuals examined at the New York Juvenile Asylum, there was a reduction of the number of red corpuscles in seven. These patients received from one to ten cubic centimetres of the serum containing the antitoxin. In only three of these seven cases was there any marked reduction: in two there was a reduction of 900,000 to the cubic millimetre; one showed a reduction of 600,000, and in the other four the reduction was not over 400,000. In connection with these figures, it should be borne in mind that with the instrument employed in counting, an error of 300,000 must be allowed for. In all these cases, the blood returned to its normal condition within a week. The leucocytes were apparently unaffected by the injections, either in their number or in their morphology.

Some twenty cases of diphtheria treated with antitoxin at the Willard Parker Hospital, showed no changes in the blood. The only fall in the number of red corpuscles, was one of 300,000 per cubic millimetre. In some anæmic cases the blood count rose steadily after admission.

As control experiments, five cases of diphtheria treated without antitoxin were kept under observation outside the hospital. In these a distinct diminution of the number of red corpuscles was observed, amounting in one case to two millions. While these cases were under observation, the blood did not return to its normal condition.

I do not think we are justified in drawing any conclusions from these results, other than that the antitoxin, in diphtheria, so far as my observations go, does not cause any degeneration of the blood.

DR. J. EWING.—I have been asked to make a few remarks about the leucocytosis of diphtheria under the influence of serumtheraphy, based upon the observation of fifty-five cases of diphtheria at the Willard Parker Hospital and some experimental work in the laboratory of the College of Physicians and Surgeons.

On examining the blood of diphtheria patients I observed, after injections of full doses of antitoxin, especially in the severe cases, a marked reduction of the

number of leucocytes. To determine whether this was produced by the constituents of normal serum or antitoxin, I injected into rabbits normal horse serum and sheep serum and into others serum containing camphor. In the latter cases I found a reduction of the leucocytes which did not go beyond the error of calculation. When, however, the powerful antitoxin was used, I think I was able to see a distinct diminution in the number of leucocytes. My experiments convinced me that antitoxin, injected into normal rabbits, has a decided effect in reducing the number of leucocytes. It, however, did not kill the rabbits, or make them sick, or produce a rise of temperature, as soon after the injections their condition returned to normal.

Experimentally a reduction of 5,000 leucocytes per cubic millimeter is sufficient to produce thrombi in the visceral capillaries. In severe cases of diphtheria where the numbers and cohesiveness of the polynuclear leucocytes are very greatly increased, and where the injection of antitoxin is often followed by a reduction of 5,000 or 10,000 leucocytes per cubic millimeter it is extremely probable that similar capillary thrombi are produced.

In my investigations at the Willard Parker Hospital I was unable at any time to observe any effect of the antitoxin on the red blood cells. As a result of my study of the effects of antitoxin, I believe it does great good rather than harm, but I wish to mention the reduction of the number of leucocytes as one of its possible dangers.

DR. J. L. SMITH.—In the New York Foundling Asylum, where we have recently had an epidemic of diphtheria, the microscopic examinations made by the resident physicians revealed the presence of the diphtheria bacilli in all cases, and in most of them streptococci were also found. The disease we had to contend with, therefore, was a mixed infection, and hence the poor results obtained from the use of antitoxin, in my opinion. In fourteen out of seventeen fatal cases, death was due to broncho-pneumonia, which was probably induced by the streptococci. In four or five cases the benefit from the use of the antitoxin was very pronounced.

THE HEALTH OF THE CITY OF NEW YORK.

THE WESLEY M. CARPENTER LECTURE

BY HERMAN M. BIGGS, M. D.

No subject more vitally concerns the welfare of a community than that pertaining to its healthfulness. How the inhabitants live, and how and at what age they die, what is the degree and character of the morbility occurring among them and what are its causes, are most important questions. They are essential features in the problem whose solution will teach men how they may live longer, healthier, and therefore happier lives.

The healthfulness of any community or locality is to be judged by the morbility and mortality statistics considered in relation to the causes of sickness and death. It is not simply the number of deaths or the cases of sickness in proportion to the population, but also the nature of the diseases which cause the morbility and mortality.

No questions in public medicine are more important than those dealing with the causes of death and death rates, and none are more complex or demand more careful study. The problems presented in a city like New York are unusually difficult because of the greatly diversified and cosmopolitan character of the population. The presence of large numbers of foreign born inhabitants of many different nationalities, grouped often in more or less restricted localities, and retaining their native customs and modes of life, and the great variations in the density of the population constitute factors which greatly complicate the situation.

In rural districts the healthfulness of the inhabitants depends largely on natural conditions, such as elevation,

climate, and soil, and to a much less extent upon the artificial conditions produced by the hand work of man. In densely populated cities on the contrary the natural conditions become relatively unimportant factors in determining the degree of healthfulness, so much do they become subordinated to the artificial conditions resulting from the lives and labor of the inhabitants. It may be said, with certain limitations, that the inhabitants of any city have it largely within their power to determine what degree of healthfulness their city shall have.

A high morbility and a high mortality in any urban population depend largely upon the objectionable character of the general sanitary conditions, such as the occupations, habitations, food and water supply of the inhabitants, all factors which lie to a great extent within their control. Hence, when any city has a high annual death rate, for example, thirty to thirty-three per thousand (as was formerly the case in Liverpool and in New York) in place of a death rate of eighteen or twenty per thousand, as exists in London, it is because of the conditions permitted to exist, to a very small extent from necessity but largely from neglect and indifference on the part of the people, or incompetence and ignorance on the part of the authorities. The expectation of life at birth or the mean life term in Liverpool and Manchester has been but little more than one half that of the healthiest districts of England. It was until recently twenty-six years in Liverpool and Manchester, thirty-seven in London, and forty-six in Surrey. Even at the age of thirty the expectation of life is seven years more for England as a whole than it is for Manchester. These considerations show what vast and practical importance to the individual at every age and to the community as a whole its healthfulness has, for upon this depends not only the relative freedom from illness, but also the mean life term of each of its members.

In the casual consideration of the healthfulness of com-

munities as compared with the conditions existing in earlier centuries we are inclined to think, without investigation, that there has been but little advance in modern times, and that while some diseases have been greatly restricted or entirely stamped out, these have been replaced by others equally as fatal to the human race. A little study, however, shows how erroneous are these views.

In the seventeenth and eighteenth centuries, the average annual death rate per thousand living throughout the civilized world was at least forty or fifty and probably much higher. From 1628 to 1635 in London, these years being free from pestilence, the average death rate was fifty per thousand and the absolute annual mortality for twenty-four years, from 1620 to 1643 inclusive, was over seventy per thousand (Farr). The mortality during 1894, in London, was only seventeen and a fraction per thousand. The mean expectation of life in London for the decennial period 1728 to 1737 as calculated by Mr. Simpson, was 19.2 years, and from 1771 to 1780, by Dr. Price's tables, 19.6, the annual mortality being fifty-one or fifty-two per thousand; while from 1831 to 1835, including an epidemic year, the death rate had already decreased to an estimated thirty-two per thousand. After a little more than one hundred years the mean expectation of life at the time of birth has increased so that for all England it is 40.8 years and for London thirty-seven years. From 1771 to 1780 not less than five in a thousand died annually of smallpox in London; from 1801 to 1810 this mortality had sunk to two per thousand; from 1831 to 1835 to 0.83 and in recent years to an insignificant fraction. Fever as a cause of death declined in nearly the same ratio from 6.21 in the decennial period 1771 to 1780, to 2.64 in 1801 to 1810, and 1.14 in 1831 to 1835. In the sixteenth century fever, plague, cholera, and dysentery destroyed annually 31 out of every 1,000 of the inhabitants in London, or nearly twice the total deaths now from all causes. Ac-

cording to the English life-tables up to 1875, 518 out of every 1,000 children born in Liverpool died in the first ten years of life, 351 died in London and 205 in the healthy districts of England; while throughout the whole of England 297 out of every 1,000 died during the first ten years of life.

I do not propose in this paper to enter into a detailed analysis of the morbility and mortality arising from different causes in different parts of New York or among different classes of the population as to age, sex, color, or nationality. Such an analysis would require far more time than is at our disposal and has already been made in a most able way by Billings in the United States census report of 1890. It would not further serve the main purpose of this paper. I desire to present in it a study of the relative healthfulness of New York as compared with its own past condition and with the past and present condition of other great cities of the world, and to point out where some of the gains have been made in the past and what we may hope to accomplish in the near future through improved hygienic conditions.

I have had prepared a series of tables (Nos. I-VI) showing the population, and the total mortality and the deaths from tubercular and zymotic diseases with the corresponding death rates for the decennial period 1884 to 1893 for a number of the great cities of the world, namely, New York, London, Berlin, Vienna, Paris, Liverpool, Manchester, and one of the health districts of England, Surrey; and also a table (No. VII) showing the average of these same data for these cities, for the ten-year period and for the year 1894. I have chosen the tubercular and so-called zymotic diseases particularly for this comparative study because these are generally recognized as preventible diseases. I have also prepared similar tables (Nos. VIII and IX) for New York city and London, showing the average mean death rate for five decennial

periods preceding 1883, calculated on the population of each of the cities as nearly as it could be obtained at the middle of each of these periods, to compare the healthfulness of the city at the present time with that of preceding years.

Before proceeding with the consideration of the data thus presented a word may be said regarding the significance of death rates. Farr says, "While the total mortality and total death rate or death toll, may be taken as a fair indication of the sanitary condition of the population under most circumstances, yet it may be very far from alone indicating the true healthfulness of the districts." Whenever the general mortality is high, agencies will be found unfavorable to the health of the inhabitants, but the mortality may be apparently low and indicate a far better sanitary condition than really exists. Mortality is generally stated at so many per cents or so many deaths per thousand living at all ages, but it is evident (Farr) that the proportion of deaths to a given number of living varies to a great extent with the ages of the living. In the first five years of life the mortality is over 7 per cent. annually; for boys at the age of ten to fifteen it is less than one half of 1 per cent.; for both sexes from fifty-five to sixty-five it is nearly 4 per cent.; from seventy-five to eighty-five it is over 14 per cent., and over eighty-five more than 30 per cent. The mortality of the two sexes also varies, so that independent of other causes of variations the mortality of different populations will differ according to the relative numbers at the different ages when the mortality rate is high, *i. e.*, under five and above fifty-five; between these periods it is comparatively low. This is true both in the healthiest districts and in cities. Infants and old people die off rapidly. The general rate of mortality, therefore, may differ in two populations merely because the one contains more of the population under five and above thirty-five than the other.

When the births exceed the deaths, as is the case in a community where the population is rapidly increasing, the effect is to increase the number of children under five. But as the births take place steadily year by year and they are in excess of the deaths, they throw into the population not only additional children under five but also children above that age and young adults, and hence there is less than the due proportion of old people. The result is that the mortality appears below what it would be in the same population, under the same sanitary conditions, if the births and deaths were equal. The death rate in males is also much higher than in females, and the predominance of one sex considerably over the other may change the rate. Immigration has the effect of reducing greatly the general rate of mortality as it swells the excess of persons at the ages of low mortality. To show the true condition, therefore, the crude death rate must be corrected as to the age and sex distribution of the population. In Liverpool when the rate of mortality was 34 the rate corrected by the English life-table was 39; in the same year the rate for London was 24, and the corrected rate 27. In 1894 in New York the rate was 22.7, and the rate corrected by the English tables was 26.48.

The disturbance in death rates in older communities arising from these and other causes, however, is not so great as to materially vitiate the value of crude annual death rates. Farr says, "In all England the crude annual death rate for thirty-five years was 22.4 per thousand," while the corrected rate by the life-table was 24.5. The underrating of the crude death rate does not exceed three or four per thousand, and this is not considerable when we find the average death rate of thirty years (1841 to 1870) varying from fifteen in Glendale to twenty-four in London, twenty-seven in the Birmingham district, and thirty-six in Liverpool. Add two to the rate for Glendale, three to Liverpool, three to Birmingham, and three to

London, and the mortality thus approximately corrected so as to make it the mortality of a population normally constituted as to age and sex becomes seventeen, twenty-seven, thirty, and thirty-nine respectively.

It will be observed that the alterations cause comparatively little change in the relations and that for immediate practical purposes relative crude mortality rates furnish a satisfactory guide. In new countries where populations are rapidly increasing in number and the increase is largely due to immigration, rates are greatly influenced by this cause, and the crude rates form a far less reliable index of the sanitary conditions. The rates in rural districts also are usually unreliable, although the defects arise here from other causes. Frequently in rural districts the recorded rates are quite impossible and incredible as indicating the true conditions.

The density of a population has generally a definite relation to the mortality. So constant is this relation, that Dr. Farr attempted to deduce a definite formula by which the mortality of any locality could be translated into that of another having a different degree of aggregation of the population. This variation has been emphasized by Ogle. In a table constructed from the population, death rates, and density of groups of districts in the years '71 to '80 he has shown that when the population has reached a certain density there is a constant and uniform increase in the death rate with increase in the density. For example, for fifty districts with a population of 2,600,000 and 650 persons to a square mile, the death rate was 20.5 per thousand. With a density of 2,100 to a square mile in twenty-seven districts with a population of 2,000,000 the death rate had increased to 24.4. With a density of 2,800 to a square mile the death rate had still further increased to 25.5 and with 6,144 persons to a square mile the death rate was 30.2. This local variation in mortality with density of population has not been constant in New York city, for in some of the wards where

the density of the population is the greatest the mortality has been below the average, and in other wards with a relatively scarce population the mortality has been extremely high. For example, in the 1st ward with about 83 persons to the acre the mortality in 1892 was 47 per thousand; in the 4th ward, with 248 to the acre, the mortality was 409, and in the 7th ward, with 335 to the acre, the death rate was 25.2; in the 11th ward, with 454 to the acre, the mortality was 22.5; in the 10th ward, with 622 to the acre, this being the most densely populated district in the civilized world and the most densely populated ward in New York city, the death rate was only 20.5, and in the 13th ward, with a density of 515 per acre, the death rate was 18.8, the lowest of any ward in the city, excepting the 2d ward, in which the population is so small, only 1,613, as to be fairly thrown out of consideration. It is evident from this that some other factors have so much predominated in these different areas in their influence on the death rate as to apparently nullify the effect of aggregation.

The highest death rates have been found to be in the lower and oldest parts of the city where the buildings are very old and the sanitary conditions in many respects bad; as in the 1st, 4th, 14th, 5th, and 6th wards, all of which are situated below Houston street. The character of the population has undoubtedly a definite influence in determining mortality. In the 12th, 22d, and 19th, 20th, and 21st wards, comprising the best portions of the city, including, it is true also, a large tenement population, the mortality is nevertheless relatively high, considerably higher than in the densely populated 10th, 11th, and 13th wards on the lower East side.

In a careful analysis of the relation in New York city of density of population to death rates, contained in a report to the tenement-house commission, Dr. Roger S. Tracy, registrar of vital statistics, showed that in New

Liverpool. The tubercular death rate is distinctly lower than that of Paris or Vienna, but is higher than that of London or Berlin. It will also be seen that the reduction in the rate has been greater in New York than in Paris or Vienna, and has been about the same as in London, and much less than in Berlin.

The rates in Surrey have been introduced to show the very striking difference in the rates prevailing in a healthy, extra-metropolitan district, as compared with the rates in the large cities. The population of the district of Surrey in 1891 was about 575,000. This district includes three or four towns each having about 50,000 inhabitants, a number of smaller towns, and a large rural population. As will be seen the average death rate for Surrey for the decennial period ending in 1893 was less than one half that prevailing in New York previous to 1873.

The table further shows that the density of population in New York below the Harlem river is far greater than that of any of the other cities included in the table. This is of course excepting Surrey, where the average population is only about one to the acre as contrasted with 143.2 to the acre in New York below the Harlem river.

There are certain districts of New York which are more densely populated than any districts in any other city in the world. The only districts in any European cities approaching these are a small area in Paris, where the population is 434.19 to the acre; one district in Prague, where the population is 485.4 to the acre; and the White Chapel district in London which has a population of 291.6 in Spitalfields, 303.5 in Mile End and New Town, and 365.3 in Bethnal Green.

In New York, ward 11, sanitary district "A," has a population of 800.4 to the acre; ward 10, 643.8 to the acre; ward 13, 539.5 to the acre; ward 17, 431.4; ward 7, 360.7, and ward 14, 295.4.

A comparison of the mean total death rates for decennial

periods in New York city since 1834 (Table IX) shows that there was an increase during the first three periods ending in 1863, and that since that date there has been a continuous and very heavy decline in the rate. In the decennial period ending in 1843, the mean death rate was 31.03; for the next period, ending in '53, it had risen to 33.81; for the next period, ending in '63, it was 33.94,—since that time it has declined to 31.11 in '73, 26.87 in '83, 25.78 in '93, and in the year 1894 to 22.76. The population meanwhile has increased from 312,000 in 1840 to 1,853,000 in 1894. The mortality in the decennial periods ending in '53 and '63 was increased by the very large mortality occurring in the epidemic years of '49 and '54. As will be seen, the death rate in New York has declined one third since 1863.

The percentage of mortality occurring in children under five is always high and has been long regarded as an excellent index of existing sanitary conditions. The injurious effect of unsanitary conditions and surroundings always falls heaviest upon the youngest element of the population. Table IX shows the death rate under five for 1894, and for decennial periods since 1860, when the health department was organized. The records in regard to this point do not extend back of this date.

It will be seen that the average death rate in children under five for the eight years terminating in '73 was 123.3; in the second period, terminating in 1883, it was 104.69; in 1893, 95.09, and in 1894, 85.32. The rate for 1894 was nearly forty points under the rate prevailing during the eight-year period ending in 1873.

The excessive mortality prevailing in New York previous to 1866 so alarmed the inhabitants as to lead to the establishment of the health department as at present organized. A large part of this excessive mortality, as will be seen, fell upon the children under five, for the mortality occurring in persons over five, while it was relatively high

and is still higher than it should be, was not so excessive. The greatest diminution in the rate since 1866 has occurred among the first portion of the population. The death rate among persons over five for the period ending in 1873 was 17.23; for the period ending in 1883, 16.43; in 1893, 16.92, and in 1894, 14.73; the reduction in this case having been less than 15 per cent., while the reduction in the rate under five was considerably over 30 per cent.

In looking for the causes of diminished mortality, investigation shows that there has been a very large and continuous reduction in the zymotic death rate including diarrhœal diseases of children under five since 1873, and also a steady and important decline in the death rate from phthisis since 1884. The reduction in the phthisis rate, however, has been largely counterbalanced by the steady increase which has occurred in the death rate from pneumonia and bronchitis[1]. The increase in this rate just about counterbalances the decrease in the phthisis rate. The remainder of the reduction in death rates is distributed generally throughout the list of causes of death; in only a few classes has there been a relatively insignificant increase, which is in part, at least, due to the more accurate diagnosis and classification of the causes of death.

In order that a satisfactory comparison shall be made between the London and New York rates, there must be a correction of these rates. It is impossible to correct the New York rates from any American standard as no life tables have been prepared in this country, but we may use the English standard life table for this purpose.

The crude death rate of New York city for 1894 was 22.76; the rate corrected by the English life-table is 26.46. (Table X.) This correction is for differences in sex and age distribution in the population as compared with the population of the whole of England and Wales.

[1]Increase due to la grippe.

The correction for New York as thus shown is 3.7 per thousand.

The crude death rate for London for 1894 was 17.76. This was an unusually low rate and is three points lower than the rate for the preceding year. When this rate is corrected it becomes 18.93, the correction in London being only 1.17.

The correction for New York is large because New York has an unusually numerous population at the middle periods of life when the natural death rate is low. The large percentage of the population at these periods is undoubtedly due to extensive immigration to the city of persons in the early working period of life. The great differences in the correction for New York and London illustrate what has previously been said, namely, that the crude rates are a far less reliable index of the sanitary conditions in new countries or in cities where the population is very rapidly increasing in number than in older communities.

The sex and age distribution of the population in New York estimated for July 1st, 1894, from the police census of April, 1895, with the English standard death rates at corresponding ages and the number of deaths in New York of males and females estimated in accordance with these rates as compared with the actual death rate, is shown in Table X.

It will be seen that the number of deaths in New York in 1894, if the rate for England and Wales had prevailed, would have been 29.792, and the death rate 16.47, while the corrected rate was 26.46, and the actual number of deaths was 41.175.

In New York 11,383 more deaths occurred in 1894 than would have occurred if the death rate for New York for 1894 had been that which prevailed in England and Wales. This standard, however, is not a just one for comparison as to the healthfulness of New York, because the English standard represents the average for the whole

population of England and Wales both urban and rural, and the urban rate is universally higher than the rural.

The rate for London affords a fairer standard for comparison. This was in 1894 after correction, 18.93. This, however, is the lowest rate for some years, in London, and is three points lower than the rate for the preceding year. The deaths in New York in 1894 as computed upon this rate would have been 34,247, or 6,928 less than the actual number. This is far from a favorable showing for New York, but in making such a comparison as this we must remember that London has not a very dense population. The city extends over a very large area and its density is only a little more than one third that of New York below the Harlem river. In London the density is 58.2 per acre, whereas in New York in the region below the Harlem river, it is 143.2. It should be further noted that the rate in London is not only the lowest of the great cities of the world, being lower even than that of Berlin, but is lower than the rate prevailing in most of the large towns, both on the continent and in England. It differs but little from the standard rate for England and Wales and is no higher than prevails in many of the best country districts of England. The death rate in London may be regarded as the best result that sanitary science has as yet been able to produce in a great city.

It may be instructive in this connection to consider for a moment in what class of disease and in what periods of life the excessive mortality in New York as compared with London occurred. Table X shows the latter at once. The excess of deaths during the first ten years of life, in New York over the English rate, is 6,372; this accounts for more than 55 per cent. of the total excess for all ages. The rate for this period is 50 per cent. in excess of the English rate. Nearly 6,000 of this excess occurred in the first five years of life. In the period from 10 to 20 the excess is only 113, or about 10 per cent. of the rate

at those ages; in the two periods, 20 to 25, 25 to 35, the excess is 1,825, or almost 45 per cent.; in the next three periods from 35 to 45, 45 to 55, 55 to 65, the excess altogether is 2,840, or about 35 per cent.; from 65 to 75 the excess is 405, or about 20 per cent.

It is only in the last two periods of life, ages from 75 to 85 and over 85, that the rates are lower for New York than London, the deaths being 1,245 to 1,163 between the ages 75 and 85, and 410 to 316 over 85, respectively.

A consideration of the classes of deaths which are markedly in excess in New York as compared with London shows first, that the death rate for zymotic diseases is very much higher, the death rate in New York for zymotic, tubercular, and diarrhœal diseases under five, for the decennial period ending in 1893, being 9.23, while in London it was only 5.80, 3.43 points lower. The very great excess in New York over London of deaths from zymotic and diarrhœal diseases under five accounts for a large proportion of the excess of death taking place at this period of life.

We find that the next largest excess in New York as compared with London occurs in the deaths from pulmonary diseases, particularly from tuberculosis and from pneumonia and bronchitis. The excess from tuberculosis falls almost entirely upon the population at the middle period of life and is largely responsible for the excess at this time. The deaths from pneumonia and bronchitis are in part accountable for the excess of deaths under five, and in part accountable for the excess of deaths during the period from 30 to 50 years of age.

The New York rates also show a marked excess in the middle and later periods of life in deaths due to diseases of the urinary organs, particularly diseases of the kidney. The remainder of the excess is rather generally distributed throughout the list.

It may be interesting in conclusion to speculate briefly

upon what may be reasonably expected in the future as to the sanitary conditions of New York as indicated by its death rate. We must remember in this connection how unfavorable the conditions are in New York in some respects, as a result of the unavoidable density and of the character and habits of the foreign-born and tenement-house population. While as has been previously shown, density of population alone in restricted localities does not constantly produce a high rate in New York, or at least not the highest rate, yet unquestionably the aggregation of the population as a whole has a large influence in the production of the comparatively high total rate.

The increased death rate arising from the density and the nationality of the population constitutes factors in the problem which at best sanitary science alone cannot entirely correct. Again, the housing of the great tenement-house population in New York is in many respects the most unfavorable existing in any great cities in the civilized world. The climatic conditions, the rapid changes of temperature, and the great extremes of heat and cold, are also important factors in influencing the death rates and are quite beyond control. In these respects, London has a definite and permanent advantage, and it is extremely doubtful whether these advantages can ever be entirely overcome and the death rate of New York reduced to the rate prevailing at the corresponding period in the English metropolis.

An inspection of the table showing the mean death rate for decennial periods in New York shows that there has been a fall of nearly 30 per cent. in the crude rate of New York in a little over twenty years, in spite of the rapid increase of the population in both density and number. The conditions now existing are to be regarded as very far from representing the best obtainable, and there is every reason to believe that the fall in the rate which has been going on for the last twenty or twenty-five years will continue. The

tubercular and zymotic death rates combined in 1894 were 7.32, out of a total of 22.76. It is evident that there is abundant room for reduction in this rate.

In the year 1894, diphtheria and croup alone caused nearly 2,900 deaths, or 7 per cent. of the total mortality. In Paris during the last year, the mortality from these diseases was reduced at one stroke to less than one quarter of the average mortality of the previous five years, and to considerably less than one third of the lowest mortality that had occurred during these five years. We may reasonably hope eventually in New York to accomplish something comparable to this, so far as diphtheria is concerned, and to thus deduct at least one point from the rate.

The mean death rate for phthisis for the decennial period ending in 1893 was 3.94, and for the year 1894, 3.17. The death rate for 1894 from phthisis was 20 per cent. less than the mean death rate for this disease for the period ending 1893.

It should be possible in the next ten years by improved hygienic methods and the further education of the public as to the infectious character of tuberculosis to reduce this rate at least 30 per cent., and thus one point more will be subtracted from the death rate.

The death rate from diarrhœal diseases under five was 1.5 in New York in 1894, and the total deaths under five numbered 17,558, and constituted more than 40 per cent. of all deaths. The death rate in London from diarrhœal diseases for this period of life was .37 and the total relative mortality was nearly 30 per cent. less.

It is not expecting too much to look for a reduction of the rate at this period of life of at least one third during the next ten years, aside from the reduction in the deaths from diphtheria and tuberculosis, under five, and this affords us nearly another point and the crude rate becomes a fraction under twenty.

Certainly the excessive mortality in some wards can be

reduced by measures directed to the removal of the local causes of high mortality, and by the improvement in the general sanitary conditions. The removal of old houses and the erection of new model tenements, the improved water supply, and the more careful supervision of milk and food cannot but reduce slowly but certainly the general mortality.

While we must remember that the rate for 1894 in New York was exceptionally low, the lowest almost in the history of the city, and that so low a rate cannot perhaps be continually maintained, yet the decennial period ending 1903 will probably show a mean death rate of less than 22, or a reduction of at least three points on the rate obtaining for the ten-year period ending in 1893. This would give a lower mean decennial death rate than has been recorded for any single year since 1820, and I have no doubt in some of the years during this decennial period the rate will fall below twenty per thousand. An abiding faith in the great value and in the truth of the discoveries of modern medicine, and a confident assurance that future investigations will advance still farther our knowledge, justify the most sanguine hopes of rapid progress in sanitation; and while we cannot but congratulate ourselves that the mean expectation of life at birth in New York, as judged by the rate of 1894, has increased in a little more than thirty years from less than twenty-eight years to more than thirty-six years, eight years having been added, still we may hope that another decennial period will add still more and that New York may come to be regarded, not as was once the case, as an unhealthy city, but as one having an unusual degree of healthfulness.

Men would often sacrifice all their possessions to have a few years added to the span of life. The improvement in the sanitary conditions in New York during the last thirty years has already added many years to the mean life time of its inhabitants, and if continued will add more.

In conclusion, I desire to acknowledge my great indebtedness to Dr. Roger S. Tracy, the able registrar of vital statistics of the health department, and to Mr. Alfred L. Beebe and Dr. J. H. Huddleston for valuable assistance in the collection of the statistics embodied in this paper.

I desire further to express my appreciation of the honor conferred on me by the invitation of the council of the Academy to deliver the Wesley M. Carpenter lecture before the Academy for 1895.

NEW YORK, LONDON, SURREY, PARIS, VIENNA, BERLIN.

TOTAL POPULATION AND DEATHS FROM VARIOUS CAUSES, WITH DEATH RATES, FOR THE DECENNIAL PERIOD 1884–1893, AND FOR 1894.

POPULATIONS.

TABLE I.

YEAR.	NEW YORK.	PARIS.	VIENNA.	LONDON.	BERLIN.	SURREY.
1884.....	1,356,764	2,239,928	759,849	3,940,042	1,242,407	496,537
1885.....	1,396,388	2.239,928	769,889	3,979,160	1,279,599	507,922
1886.....	1,437,170	2,239,928	780,066	4,018,666	1,335,667	519,533
1887.....	1,479,143	2,260,945	790,381	4,058,565	1,385,991	531,374
1888.....	1,522,341	2,260,845	800,836	4,098,860	1,439,617	543,470
1889.....	1,566,801	2,260,945	811,434	4,139.555	1,489,672	555,758
1890.....	1,612,559	2,260,945	822,176	4,180,654	1,548,279	563,080
1891.....	1,659,654	2,424,705	1,378,530	4,222,157	1,601,327	575,078
1892.....	1,708,124	2,424,705	1,406,932	4,264,076	1,662,237	587,313
1893.....	1,758,010	2,424,705	1,435,931	4,306,411	1,714,938	600,052
1894.....	1,809,353	2,424,705	1,465,537	4,349,166	1,701,643	612,849

TABLE II.

NEW YORK.

YEAR.	Miasmatic and diarrhœal diseases.	Diarrhœal diseases of children under 5 years.	Tubercular diseases.	Miasmatic amd diarrhœal diseases. Diarrhœal diseases of children under 5 years deducted.	Miasmatic, diarrhœal, and tubercular diseases.
1884.....	7,892	3,160	6,039	4,732	13,931
1885.....	7,857	2,895	5,953	4,962	13,810
1886.....	8,283	2,990	6,349	5,293	14,632
1887.....	8,890	3,252	6,007	5,638	14,897
1888.....	9,020	3,051	6,073	5,969	15,093
1889.....	8,697	3,135	6,041	5,502	14,738
1890.....	7,108	2,997	6,409	4,111	13,517
1891.....	8,179	3,191	5,909	4,988	14,088
1892.....	8,073	3,162	6,061	4,911	14,134
1893.....	8,037	2,898	6,163	5,139	14,200
Total.....	82,036	30,731	61,004	51,305	143,040
Average death rate, 1884-'93.....	5.29	1.98	3.94	3.31	9.23
1894.....	7,516	2,708	5,729	4,808	13,245
Death rate.....	4.15	1.50	3.17	2.66	7.32

Miasmatic, diarrhœal, and tubercular diseases same as for London.

TABLE III.

LONDON.

YEAR.	Miasmatic and diarrhœal diseases.	Diarrhœal diseases of children under 5 years.	Tubercular diseases.	Miasmatic and diarrhœal diseases. Diarrhœal diseases of children under 5 years deducted.	Miasmatic, diarrhœal, and tubercular diseases.
1884.....	14,537	3,609	12,537	10,928	27,074
1885.....	12,124	2,440	11,850	9,684	23,974
1886.....	11,870	3,718	12,164	8,152	24,034
1887.....	13,426	3,498	11,424	9,928	24,850
1888.....	11,423	1,992	10,923	9,431	22,346
1889.....	10,290	2,449	11,168	7,841	21,458
1890.....	12,728	2,479	12,306	10,249	25,034
1891.....	10,158	2,188	11,854	7,970	22,012
1892.....	12,313	2,319	11,465	9,994	23,778
1893.....	13,177	3,136	11,426	10,041	24,603
Total.....	122,046	27,828	117,117	94,218	239,163
Average death rate, 1884–'93.....	2.96	0.68	2.84	2.29	5.80
1894.....	11,742	1,615	10,570	10,127	22,312
Death rate.....	2.70	0.37	2.43	2.33	5.13

Miasmatic diseases include: Small-pox, measles, scarlet fever, diphtheria, croup, typhus, typhoid, simple and ill-defined fevers, and whooping cough.

Diarrhœal diseases include: Cholera, diarrhœa, dysentery.

Tubercular diseases include: Tabes mesenterica, tubercular meningitis, phthisis, other forms of tuberculosis, scrofula.

TABLE IV.

	SURREY* (Ex-Metropolitan).			PARIS.		
YEAR.	Miasmatic and diarrhœal diseases.	Tubercular diseases.	Miasmatic, diarrhœal, and tubercular diseases.	Miasmatic and †diarrhœal diseases.	Tubercular diseases.	Miasmatic, †diarrhœal, and tubercular diseases.
1884.....	1,078	1,077	2,155	6,964	11,625	18,589
1885.....	930	1,110	2,040	5,080		
1886.....	1,009	1,098	2,107	5,168	12,258	17,426
1887.....	842	1,032	1,874	6,342	11,818	18,160
1888.....	609	1,094	1,703	4,531	11,472	16,003
1889.....	729	1,143	1,872	4,963	12,047	17,010
1890.....	890	1,144	2,034	4,948	12,586	17,534
1891.....	741	1,144	1,885	3,795	12,430	16,225
1892.....	917	1,070	1,987	4,989	11,153	16,142
1893.....	1,154	1,072	2,226	3,907	12,267	16,174
Total.....	8,899	10,984	19,883	50,687	107,656	123,263
Average death rate, 1884–'93.....	1.63	2.00	3.63	2.20	‡4.66	6.63
1894.....	775	960	1,735	3,643	12,376	16,019
Death rate.....	1.26	1.57	2.83	1.50	5.10	6.60

NOTES.—Diarrhœal diseases of children under five years not obtainable.

* Diseases included are the same as for London.

† Diarrhœal diseases of children not included, as these are combined with marasmus in the tables. Diarrhœal diseases include cholera and cholera nostras only.

‡ Average is for nine years only; tubercular deaths for 1885 not obtainable. Tubercular and miasmatic diseases, same as for London.

TABLE V.

VIENNA.

YEAR.	Miasmatic and *diarrhœal diseases.	Diarrhœal diseases of children under five years.	Tubercular diseases.	Miasmatic and diarrhœal diseases. Diarrhœal diseases of children under five years deducted.	Miasmatic, diarrhœal, and tubercular diseases.
1884	2,452	2,295	5,470	1,157	7,922
1885	3,604	1,567	5,574	2,037	9,178
1886	3,127	1,530	5,525	1,597	8,652
1887	3,143		5,110		8,253
1888	2,745	1,392	5,054	1,353	7,799
1889	3,006	1,719	4,802	1,287	7,808
1890	3,101	1,578	4,876	1,523	7,977
1891	6,347	3,085	7,940	3,262	14,287
1892	6,728	3,560	7,745	3,168	14,473
1893	6,467	2,978	7,418	3,495	13,885
Total	40,720	‖18,704	59,514	‖18,879	100,234
Average death rate, 1884–'93	4.17	1.88	6.10	1.90	10.30
1894	†6,132	†2,945	‡	3,187	
Death rate	4.18	2.01		2.17	

*Diarrhœal diseases include dysentery, cholera, and inflammation of the stomach and intestinal tract.

Miasmatic and tubercular diseases, same as for London.

† From the Paris reports.

‡ 1894 phthisis only 6,661. All tubercular diseases not obtainable.

‖ Nine years only. 1887 not obtainable.

TABLE VI.

BERLIN.

YEAR.	Miasmatic and *diarrhœal diseases.	Diarrhœal diseases of children under five years.	Tubercular diseases.	Miasmatic and diarrhœal diseases. Diarrhœal diseases of children under five years deducted.	Miasmatic, diarrhœal, and tubercular diseases.
1884.....	7,964	3,509	4,534	4,455	12,498
1885.....	6,566	2,990	4,657	3,576	11,223
1886.....	7,514	4,260	4,547	3,254	12,061
1887.....	5,841	3,142	4,388	2,699	10,229
1888.....	5,082	2,754	4,554	2,328	9,636
1889.....	7,134	4,557	4,938	2,577	12,072
1890.....	7,293	3,250	4,684	4,043	11,977
1891.....	5,857	3,752	4,749	2,105	10,606
1892.....	5,957	3,313	4,357	2,644	10,314
1893.....	7,897	4,542	4,680	3,355	12,577
Total.....	67,105	36,069	46,088	31,036	113,193
Average death rate, 1884–'93.....	4.57	2.45	3.14	2.11	7.70
1894.....	†7,639	†4,274	‡	†3,365	
Death rate.....	4.49	2.51		1.98	

* Diarrhœal diseases include dysentery, cholera, and entereo-colitis (stomach and intestinal inflammation, stomach and intestinal catarrh).

Miasmatic and tubercular diseases same as for London.

† From the French reports.

‡ 1894 phthisis only 3,810. (All tubercular diseases not obtainable.)

TABLE VII.

Comparison of Death Rates, New York and Certain Foreign Cities, for the Decennial Period 1884-'93, and for 1894.

Death Rate.		New York.	London.	Manchester.	Liverpool.	Surrey.	Paris.	Vienna.	Berlin.
Total,	1884-'93	25.78	20.40	26.20	26.00	15.48	23.93	25.57	23.48
"	1894	22.76	17.80	20.40	23.80	.....	21.32	24.56	17.57
Miasmatic and Diarrhœal Diseases,	1884-'93	5.29	2.96	.....	.....	1.63	*2.20	4.17	4.57
	1894	4.15	2.70	.....	.....	1.26	*1.50	4.18	4.49
Diarrhœal Diseases of children under 5 years,	1884-'93	1.98	0.68	.....	.....	.....	.....	†1.88	2.45
	1894	1.50	0.37	.....	.....	.....	.....	2.01	2.51
Tubercular Diseases.	1884-'93	3.94	2.84	3.19	.....	2.00	†4.66	†6.10	3.14
	1894	3.17	2.43	2.82	.....	1.57	5.10	.....	.....
	1896	3.01							
Miasmatic and Diarrhœal Diseases; Diarrhœal Diseases of children under 5 years deducted,	1884-'93	3.31	2.29	3.55	3.74	.....	.....	†1.90	2.11
	1894	2.66	2.30	2.38	3.41	.....	.....	2.17	1.98
Miasmatic, Diarrhœal, and Tubercular Diseases.	1884-'93	9.23	5.80	.....	.....	3.63	*†6.63	†10.30	7.70
	1894	7.32	5.13	.....	.....	2.83	*6.60	.....	.....

*Diarrhœal diseases include cholera and cholera nostras only.

†Average for nine years only.

TABLE VIII.

LONDON.

Death Rates from Various Causes for Decennial Periods 1844-1893, and for 1894.

		1844-'53	1854-'63	1864-'73	1874-'83	1884-'93	1894
Total Death Rate.....		24.55	23.84	24.10	22.05	20.48	*17.80
Cholera	Death Rate	0.73	0.48	0.25	0.028	0.024	* 0.007
	Per cent. of Total Mortality.....	2.97	2.01	1.04	0.13	0.12	0.04
Small-pox	Death Rate.....	0.41	0.27	0.49	0.23	0.07	* 0.02
	Per cent. of Mortality.....	1.67	1.13	2.03	1.04	0.34	0.11

*For the fifty-two weeks ending, December 29, 1894.

TABLE IX.

NEW YORK.

Death Rates from Various Causes for Decennial Periods, 1834-1893, and for 1894.

		1834-'43	1844-'53	1854-'63	1864-'73	1874-'83	1884-'93	1894
Total death rate		28.03	33.81	33.94	*31.11	26.87	25.78	22.76
Death rate under five years, ...		.... ..	.....	.....	†123.31	104.69	95.09	85.32
Death rate over five years, ...		.....	.....	.....	†17.23	16.43	16.81	14.73
Cholera	Death rate	0.32	1.19	0.36	0.15	0.15	0.006	0.00
	Per cent. of total mortality,	1.14	3.52	1.06	0.48	0.00	0.023	0.00
Small-pox	Death rate, ...	0.61	0.77	0.47	0.41	0.25	0.03	0.09
	Per cent. of total mortality	2.18	2.28	1.38	1.32	0.93	0.12	0.40

*Health department, as at present constituted, inaugurated May, 1866.
†Eight years only; 1866-'73.
The death rates prior to 1851 are below the actual rates, as the registration of deaths, where burials occurred within the city limits, was not required by law.

TABLE X.

Age	July 1st, 1894. Population New York City. Males.	Females	Total.	English standard death rates. M.	F.	Deaths in New York with a corresponding rate. Male.	Female	Total.	Actual deaths N. Y. Total.
0	102,809	102,964	205,773	61.6	51.9	6,333	5,344	11,677	17,558
5	80,996	81,509	162,505	5.4	5.3	437	432	869	1,356
10	68,740	71,151	139,891	3.0	3.1	206	221	427	469
15	76,728	89,841	166,569	4.3	4.4	330	395	725	806
20	94,987	118,260	213,247	5.7	5.5	541	650	1,191	1,646
25	198,827	190,141	388,968	7.8	7.4	1,551	1,407	2,958	4,328
35	136,300	117,185	253,485	12.4	10.6	1,690	1,242	2,932	3,966
45	81,540	75,194	156,734	19.4	15.1	1,582	1,135	2,717	3,765
55	38,384	39,799	78,183	34.7	28.5	1,332	1,134	2,466	3,224
65	15,561	17,884	33,445	70.4	60.4	1,095	1,080	2,175	2,578
75	3,874	5,182	9,056	146.6	130.6	586	677	1,245	1,163
85	556	888	1,444	305.8	270.8	170	240	410	316
	899,302	909,998	1,809,300			15,835	13,957	29,792	41,175

Death-rate (standard) 1894, 16.47
Factor of correction for New York, $= \frac{19.15}{16.47} = 1.1627$.

Deaths in New York, 1894, . 41,175
Death-rate 22.76
Corrected death-rate, . . 26.46

LONDON.

Standard death-rate, 17.97.
Factor of correction, 1.0656.
Death-rate, 1894, 17.76.
Corrected ditto, 18.93.

TABLE XI.

NEW YORK CITY.

Death Rates from Various Diseases with Total Death Rate by Wards, arranged according to Total Death Rate.

1894.

WARD.	Population.	Density of popu. per acre.	Death-rate; Total.	DEATH RATES.				Death-rate; miasmatic and diarrhœal diseases, phthisis, pneumonia, and bronchitis.
				Miasmatic diseases.	Phthisis.	Pneumonia and bronchitis.	Diarrhœal diseases.	
2	1,015	13.3	40.4	2.96	2.96	6.90	0.00	12.8
14	31,185	295.4	36.3	5.32	3.91	9.65	2.60	21.5
1	12,226	72	34.8	3.68	5.23	5.07	2.13	16.1
4	17,990	220.9	34.7	3.28	4.89	6.73	2.89	17.8
5	10,364	66.2	32.7	3.57	3.47	5.79	2.51	15.3
3	3,923	38.6	28.3	2.29	2.55	2.04	0.76	7.7
8	3,667	177.2	28.3	3.49	3.82	4.92	1.89	14.1
6	22,381	226.5	27.1	1.99	5.09	5.72	1.39	15.2
9	59,613	200	26.2	3.19	3.25	3.34	2.05	11.8
19	261,059	144.3	25.8	3.37	2.45	3.42	2.03	11.3
20	92,829	226.8	25.2	2.82	3.46	3.52	1.66	11.5
18	65,949	134.9	24.7	3.08	2.97	3.70	1.56	11.3
21	70,519	189.9	23.0	2.23	2.65	3.02	1.33	9.2
16	56,136	180.6	22.8	2.23	2.39	2.96	1.44	9.0
15	25,625	116.5	21.6	1.95	2.93	3.39	1.05	9.3
7	72,555	360.7	21.5	2.58	2.27	3.79	1.78	10.4
12	356,201	61.6	20.8	2.75	2.10	2.79	1.57	9.2
17	112,142	431.4	20.8	2.55	2.74	3.05	1.54	9.0
22	190,502	115.9	20.6	2.30	2.44	3.00	1.43	9.2
23	79,729	19.1	20.5	3.29	2.50	2.65	1.73	10.2
10	68,587	643.8	20.3	2.71	2.23	3.11	1.72	9.8
24	25,911	3.3	18.6	2.47	2.20	2.32	1.27	8.3
11	84,768	407.1	18.2	2.58	2.06	2.94	1.80	9.4
13	57,477	539.5	18.1	2.87	1.53	2.99	1.36	8.8
AVERAGES.								
Total,	1,809,353	71.7	22.76	2.74	2.57	3.35	1.68	10.34

Anniversary Meeting, November 24th, 1895.

THE PROGRESS OF MEDICINE.

By EDWARD G. JANEWAY, M. D.,

NEW YORK.

MR. PRESIDENT, MEMBERS OF THE ACADEMY, LADIES, AND GENTLEMEN: It is my privilege to address you this evening, and while I have felt that some one with more oratorical power might have been chosen, yet it was not possible to refuse the request of the President, who put it in the light of an unavoidable duty. In going over the ground for a subject which might be of general interest, and yet more, one from which lessons of importance to the welfare of the Academy might be drawn, the speaker has selected the progress of medicine. It is not the intention to pass in review the gradual advances of medical science, but to limit our theme to a time sufficiently recent to be within the memory of a number, and the knowledge of all—that of the last thirty years. The theme will be somewhat discursively considered, in order to touch on a number of points.

If we commence with the study of medicine as now conducted in this city, and compare it with that which obtained thirty years ago, we will notice a marked improvement, which, however, is not perfection, because of certain limitations of time, and of number of students to be instructed within that period. Thirty years ago the time required for the study of medicine was three years, comprising two courses of lectures in a medical school, the last of which was to be in that institution which issued the

diploma. A considerable part of the study and of the practical work was supposed to be done in the office of a practitioner with whom the student entered, and who was usually too busy to give more than a very slight oversight on the progress of the aspirant for medical honors. Much of the work in those days, far more than at present, when there is yet too much, was in the direction of what is called teaching, cramming, etc., consisting in making the student memorize facts from books and lectures, and not education in its truest sense. The idea of the difference between a true educator and a teacher in the ordinary acceptation of the word, does not enter into the conception of many medical students, and is possibly absent from the sphere of some teachers of medicine. In the coming era the demand will be more and more for educators, not for the ordinary teacher, who will simply cram the student with facts, theories of disease acceptable to himself, and plans of treatment of which he approves, as the only possible things which the student must remember. The facts may not all exist, the theories be false, and the plan of treatment one which is but the fad of the present, or possibly even of the past, to be discarded later; nevertheless the student is obliged to learn these things. How many a student in after life has wondered at the difficulty of shaking off false conceptions of disease because of the thoroughness with which they had been ingrafted during the stage of pupillage! How different this plan of dealing with the youthful medical mind from that which should obtain if the best growth of which each is capable is to be obtained. The true educator takes the student (speaking now of one branch as illustrative) to the disease as it exists in the living patients, showing him the different aspects in which it may present itself, letting the student note the condition of the mind in delirium, scan the state of the eye and pupil, and of the expression of the features, see the variation of temperature, feel the different characters of

the pulse, note the phases of eruption, listen to the varying sounds to be heard in the heart and lungs, and palpate the conditions capable of such method of demonstration in the different parts of the body. Moreover, as he trains the student to observe he will try also to impress upon him the importance of these observations by applying them to diagnosis and to treatment. But here we can discern the great and radical difference between a teacher and an educator. The former will strive to impress upon the student that such and such is the case because I say so, or it is *my* way of doing, the ego being ever so large as to obscure the true object of teaching, viz., to develop the student into a correct thinker, not a dwarfed reproduction of the teacher. On the other hand, the educator will go over the varied manifestations, striving to make the student think out first the conditions, and next the method of combating these, so that with each case seen he may ever be a better reasoner, and be more capable of recognizing such subtile differences as may determine this or that to be the preferable plan of procedure.

In the death of your late President, Dr. Alfred L. Loomis, one of the prominent teachers and educators has been removed. The curriculum of each of the medical colleges of this city (speaking for our branch of the profession) has been lengthened, until now, in one, it consists of a compulsory four years' graded course, extending over eight months of the year, with a large portion of the time devoted to laboratory work and to practical study of disease. In the two other colleges for men the compulsory course is of three years' duration, for six months of each year, with an addition of two months of permissory study under the direction of the faculty. The medical college for women has as rigid requirements as those for men.

In all the colleges much of the practical teaching for which formerly an extra charge was made is now a part of the regular course. In by-gone times students were at

times given diplomas entitling them to practise medicine, when they had never heard the morbid sounds of disease, applied a bandage, or helped at the introduction of a new life, but had only crammed the theory of these things from books. Each decade has witnessed a striking improvement in this direction. Some may say that there were clinical lectures given in all the colleges, but what did those students learn, who were on the back seats in a crowded amphitheatre, of many of the subjects presented? It should be added here that the medical colleges of other cities have also advanced in their requirements and plan of study, or have, as the Johns Hopkins, only had that method of medical instruction *ab initio*. Most of you, moreover, realize the additional burdens which have been placed upon medical students in the newer and the more extended branches of study, compared with those that obtained thirty years ago. At the present time more or less of histology, normal and pathological, is required in all our colleges, and in considerable degree this is a practical course. In one school compulsory laboratory study of bacteriology has been added. Thirty years ago the former may have been in a measure permissible, were the student so minded and could get the opportunity; of the latter practically nothing was known. Now, moreover, each of the colleges has laboratories and microscopes, the former the gift in each case of some citizen of this city who should ever have pleasure in the contemplation of having done his share in placing our medical colleges in the position of truer educational centers. Volumes are now written upon the subject of bacteriology in each language, where formerly none existed, and the importance of this new branch constantly increases. Yet there is a singular confession of our ignorance to be made here. We are as yet unable to teach the student the name, or to know ourselves the organism productive of some of the most contagious of diseases.

With the growth of knowledge in different directions there has come more or less of a necessity of dividing the teaching faculty of our colleges into those devoting themselves to the scientific subjects and those who teach what are called the practical branches. Formerly a professor might teach physiology or pathology, and at the same time practise medicine; but, owing to the rapid and widening growth in these branches, some of the colleges have the professors of these chairs devote themselves to the study of the subject and not engage in practice, in consequence of the inability to do their best work under the distractions incidental to the practice of medicine. The growth of physiological chemistry is so considerable, and the study of pathology, and especially of bacteriology, is so time-robbing, that in case the professor desires to be an educator he must devote himself to the subject. My object in thus briefly bringing before you this matter, its importance, and the necessity which exists, or will exist, in all our medical schools of having men devote their time to the consideration and teaching of these scientific subjects, instead of making them accessory to the practice of medicine, is to call attention to the necessity of urging the endowment of these chairs on scientific subjects in such colleges as have not the means of properly remunerating these chairs. The professor of what is called a practical subject is enabled to improve his position and to increase the pecuniary return from the work and time he bestows on teaching.

Hence the necessity exists, and the question comes, From whence are the colleges to obtain their endowment fund? In Germany the government provides, but in our country we must look to our wealthy citizens not alone to build colleges and laboratories, but also, what is of more importance, to endow chairs in these institutions, at least those of a scientific nature. At a recent meeting of the American Academy of Medicine resolutions were adopted

which looked toward the discouraging the building of any more hospitals and dispensaries, on the ground that enough, or more than enough, already existed, and that their effect was to interfere with the returns which the number of physicians now in this country should obtain from the practice of their profession. It may well be asked also if there are not too many medical colleges, especially those in small cities, turning out too many physicians, some of them poorly equipped for their work, but yet competitors with the better educated for a livelihood.

However, the establishment in different states of the requirement of a license from a body of examiners appointed directly or indirectly by the state, and issued only after an examination, has acted in some degree as a safety-valve, for a fair percentage of candidates fall short of the requirements. The practice of medicine, moreover, owing to a license being required in most of our neighboring states from its own board, is hedged with more restrictions than formerly, and it is a matter for consideration whether some inter-state courtesy or reciprocity will not be a necessity. New Jersey, I am told, will allow a licentiate of New York to file his certificate from the latter state and practise thereon, but he must pay double the fee over what would be required if he passed an examination.

It behooves me to speak with emphasis upon the benefit the medical profession has received from the employment of efficient trained nurses. The Bellevue Hospital training school is the parent institution, and only twenty odd years old.

Two schools now exist for the teaching of those graduates of medicine who wish to avail themselves of the rich clinical material in New York city in a short space of time. Few of us appreciate the immense strides in our knowledge of some departments of medicine. In no

branch have greater achievements been reached than in neurology. Numerous systematic treatises now exist upon the subject, several authors being members of the Academy, and it is taught as a special course, instead of, as formerly, being grouped with general medicine.

Thirty years ago multiple neuritis was unknown, the most astute observers considering the manifestations as indicating a subacute general spinal paralysis, and writing the methods of diagnosis of this complaint. To-day a medical student is readily taught the means of discrimination, and in that bygone time not a few cases of cured progressive muscular atrophy and locomotor ataxia were to be referred not to the cure of these latter conditions, but to a mistake between those conditions and a disease not then recognized. Gradually, too, in these years the term neurasthenia, first employed by a member of this Academy, has come into general use. The monotony of having only malaria for obscure states of the system has been wonderfully relieved by the complex of conditions comprised under this head, and with lithæmia they divide the debatable ground.

I fear that a number of my hearers have never read the book upon spinal-cord troubles which was much resorted to in those days, the brochure of the lamented and brilliant Brown-Séquard. In this work we were taught to distinguish between meningitis, myelitis, congestion of the cord, and reflex paralysis. This was a very simple matter compared with the terms and the diseases of the spinal cord which the student must to-day comprehend. I shall not weary such of my auditors as may not be familiar with the long list. We are indebted for the progress to many workers, each adding something to the structure, though no name stands out so prominently in the whole field as that of the late distinguished French physician, Professor Charcot. So much has he been identified, both by writing and teaching, with diseases of the nervous system, that

many are unaware that he contributed articles on the diseases of the liver and kidneys; and, in fact, after Moneret, was the next to employ the term fever intermittent hepatic of biliary obstruction, years before lectures were delivered and articles written about this condition in English; at a time, in fact, when most of our text-books were teaching that fever did not occur with biliary colic and pointed to some other condition. Pardon me for the digression, but I thought that this brief notice might be of interest.

In no direction have greater advances occurred, moreover, than in the ability to locate the area of disturbance in the brain. The fight for localization was a long and arduous one, whose beginnings date beyond this era, in the discovery of the connection between aphasia and disease of the third frontal convolution of the left side. Yet he who reads an article on aphasia of to-day, and such a masterly one as that contained in Trousseau's clinical lectures, will note the growth of our knowledge on this subject. The writer recalls the amazement of a friend well versed in neurological knowledge at the time, on noticing the record of an autopsy in a case of aphasia, in which the lesion was found in the left temporo-sphenoidal lobe, and his insistance that a mistake had been made in not noticing some connection of the disease with the third frontal.

This is an illustration of the importance of paying great attention to such cases as deviate from the general type and the ordinary phenomena of a recognized disease; for he who carefully scans all the features of such a case with sufficient knowledge may discern a new law or bring to light a subdivision of importance. We now understand word-deafness and its relation to disease of the temporo-sphenoidal lobe. Had my friend, who made the autopsy in that case, also been possessed of all the facts in the ante-mortem history, he might have been one of the earliest describers of word-deafness. Moreover, we have been

very considerably indebted to vivisection for the advance in this direction. Without this method of investigation the writer cannot see how the increase of knowledge which has occurred would have been possible. Undoubtedly it has required the careful and painstaking research of numerous autopsies on those dying of disease of the brain, to confirm in the human being the observations made on the lower animals; but these alone would not have sufficed to place the theory upon a firm foundation. The brain surgery of to-day is in consequence a very different affair from that which obtained in the sixties. In certain instances we feel that it has led enthusiasts too far, and that a certain proportion of such operative interferences might far better not have been attempted. Our distinguished ex-president, Dr. Jacobi, has drawn attention to this in his address, "Non nocere." Yet the outcome has been that lives have been saved by the ablation of tumors, by the opening of abcesses, and by the removal of clot, whose location was only recognizable by study of rules growing out of the work of Hitzig, Ferrier, Munk, and others.

The railroad spine, or concussion, has also been a product of these last thirty years, Erichsen having launched the term greatly to the regret of numerous railroad companies and corporations, who have been in many cases mulcted of large sums owing to the elasticity of the term. A disease, indeed, with varied symptoms, but having in one respect a close resemblance to dipsomania, viz., in that a certain proportion of the cases are best treated and restored by the gold cure. So, too, in this connection there was recorded, unfortunately, that first instance of abnormally high temperature in the case of a young lady thrown from her horse, and while the temperatures recorded in her case, and some more remarkable of subsequent dates, have always puzzled the writer and many of his friends, they believe them only explainable by juggling with the thermometer.

Not alone has the list of diseases of the spinal cord been increased by our expanding knowledge, but many of supposed functional character have been found to be of organic nature; the difficulty earlier having been the lack of autopsies in appropriate cases, and in addition the want of accurate methods of investigation in such a structure as the spinal cord. Within this period also the choked disk became noted as a pathognomonic sign of tumor of the brain, and though it was soon discovered that tumor of the brain might occur without this, yet it took considerably longer to dispel the idea that tumor was the cause of trouble should a choked disk be found. So much was this the case that when one had been noted, and the subsequent course of events demonstrated the inaccuracy of the statement, attempts were made to draw a distinction between the appearances in the two conditions. Now we know that the condition is not pathognomonic. However, later on we will allude to this same sort of error in disease of another organ. The first observer draws attention to the existence of a certain state in some disease, others note the same thing, and soon an enthusiast, or better, a dogmatist, announces such a condition as a sign of such a disease, and years are spent in undoing the evil.

The term myxœdema has also come into use, indicative of a disorder not dreamed of thirty years ago, and it illustrates the method of discovery of relationship between an organ and a state of the system long overlooked, and for which a remedial measure has been found in the feeding of the sheep's thyroid. The names of Gull, Ord, Horsley, and Murray stand out prominently in the gradual description, recognition, and understanding of the pathology and treatment of this state.

It has been a matter of interest, too, to note how little of apparent notice our German friends took in a diseased condition worked up by English physicians; though Kocher, of Berne, in describing the results of the removal

of the thyroid described myxœdema and helped to bring into relief the true cause of the complaint.

The method of mediate percussion introduced by Avenburger, and that of auscultation taught by Laënnec, gave, in addition to the other, though not so essential means of examination, an early insight into the diseases of the heart and of the lungs long before the period of which I write. The chief additions to our knowledge during this period have been by the explanations bacteriology has given as to the etiology of disease. Of course, pathological examinations have gradually increased our comprehension of the intimate changes of disease, but no such marked increase as has been noted in the case of the nervous system has occurred.

Actinomycosis has been made out. Explorations with needle and syringe have enabled us to tell at an early date the nature of fluids in the chest and pericardium. Moreover, I believe that this method was more employed in New York at an early period than abroad, and though liable to abuse by careless use of unclean instruments or by lack of forethought as to the nature of the process for which used, it has proved of marked benefit. The introduction of aspiration by Dieulafoy has simplified the withdrawal of fluid from the pleura and from the pericardium, while free drainage has gradually helped to cure cases of empyema in a shortened period of time. We understand, moreover, ulcerative endocarditis; have had the terms tachycardia and bradycardia replace the simple names of rapid and slow heart. Moreover, it seems to the speaker that at the present time greater attention is given to a consideration of the condition of the heart muscle, and the importance of the study of this as contrasted with that of the adventitious sounds or murmurs which were formerly so apt to monopolize attention, or else receive too considerable thought.

The use of the stomach-tube for investigation and for treatment of disorders of this viscus has been the means of

widening our knowledge of stomach disorders, and also of proving a very important aid in bringing about cure or amelioration of its diseases. This is sufficiently familiar to you, as is no doubt that other production of erroneous diagnostic rules based upon its employment. For no sooner was it found that free hydrochloric acid was absent from the stomach in cases of carcinoma, than it was assumed that the absence of free hydrochloric acid meant carcinoma. The dogmatist laid down the law on an insufficient basis, only to discover later that the methods were at times inaccurate, and that other conditions could at times produce the same result.

Perhaps no greater improvement in our consideration of abdominal disorders has taken place than in the recognition of appendicitis as a diseased condition of frequent occurrence, and that a very large proportion of what had been formerly termed perityphlitis in a loose way, was in reality periappendicitis, and that the exudate in the majority of these cases was intra-peritoneal. Moreover, it has proved the means of lessening largely the cases of so-called idiopathic peritonitis, the majority of them being dependent upon perforations or disease of this apparently unnecessary (unless to the surgeon) portion of our anatomy. A former member of this Academy, Dr. Willard Parker, laid down the rules of guidance for perityphlitic abscess—and it was reserved for another, Dr. McBurney, to urge the importance of the early operation in the cases of appendicitis. Moreover, from what has come to my knowledge here, compared with that of which I have information on this subject abroad, I believe that the surgeons and physicians of New York city and of the United States have had a much truer conception of the process and of its proper treatment than have those of other countries. In fact, they have only within a recent period waked up to a just realization of this matter. It has hardly been necessary to remind you of this advance in view of the number of times

that you have been addressed upon it. So, again, the recovery from tubercular peritonitis as a result of a simple laparotomy is a progress whose real explanation eludes us as yet.

When we come to consider the etiology of disease, and particularly of the infectious disorders, we at once appreciate our true conception of these conditions. Thirty years ago we were still as those who see through a glass, darkly, in this matter, though in the past there had ever been those who felt the need of some organized body with life and capability of division, to explain this infectiousness and contagion. Any who will look up the literature of influenza will be struck by the soundness of reasoning of some of the observers of epidemics of this disease in the last century—a soundness of reasoning that ought to put the blush on the cheek of some writers on influenza, who indulged in speculations as to the cause of the disease when it made its first reappearance some five years ago. For this purpose I can commend Symes Thompson's historical survey, and particularly the extracts from Gray and Haygarth, and the statement of Heberdeen about Haygarth's communication in reference to the outbreak of 1782. It would take too long to reproduce here Haygarth's method of reasoning in his own words, but it it is of particular interest to note that his communication in regard to the outbreak of 1782 was not published until 1803, on the occasion of another epidemic.

Haygarth gives the following interesting reason for the publication of his communication, after twenty years, regarding the influenza of 1782, and his reasons for considering the influenza to be due to contagion: "The contagious nature of the influenza had, as I thought, been sufficiently proved by many physicians. . . . But a contrary and, as I think, a very pernicious opinion, has lately been supported by physicians of great respectability, and authors of the highest reputation, not indeed in this,

but in other enlightened nations, have ascribed not only this, but many other epidemics, even the plague itself, to a morbid constitution of the atmosphere, independent of contagion. To determine whether this doctrine be true or false is of the highest importance to mankind. Knowledge in this instance is power. So far as it can be proved that a disease is produced by contagion, human wisdom can prevent the mischief. But the morbid constitution of the atmosphere cannot possibly be corrected or controlled by man."

The younger members of the Academy will not remember the long controversy regarding the possible infectious character of tuberculosis, including phthisis. There was only a small minority of physicians who admitted this prior to 1882, when Koch announced his discovery of the germ of tubercular affections. As often happens, not a few of the laity, judging from what came under their observation from time to time, had accepted the theory; moreover, there was a strange superstition, or perhaps fancy would be a better word, in different parts of the country, that in case an invalid with the disease could give the malady to an animal he might recover. This idea was undoubtedly founded upon the observation that at times one of the lower animals, in contact with a person suffering with phthisis, contracted the disease. Moreover, even after Koch had announced his discovery, and even after its confirmation by many different observers, it required still much controversy to cause its more general acceptance. As so often happens, those physicians who made a special study of the disease were found in certain instances the most difficult to convert. Especially were such arguments used as, that we have had charge of a hospital for the disease (Brompton Hospital), or a hospital in which the disease was treated, and neither our house staff, our nurses, nor our attendants, nor other inmates, were affected in any such manner as to prove the idea. This is the argument always

used by those who dislike to admit the force of reasoning relating to improving conditions dependent upon acceptance of a theory adverse to business interests, or to theories previously taught and held. As a theory it has proved dangerous, because those who have become ill in this way are not to be looked for among the present workers in places of nuisance, nor in the case cited in hospitals and wards for consumptives. They have either given up their places or have been laid where alone the tombstone will speak. The writer had in his earlier practice collected a number of instances pointing toward the dissemination of phthisis from sick to the well in families, in houses, in institutions, and in one case from one patient to four successive dogs. In coming to the conclusion some years before he published his conviction that there was a contagious element in phthisis, he found very few among the leaders of medical thought in this city who accepted the idea. And even after Koch promulgated his proofs about the tubercle bacillus, the statement was that this germ found favorable soil in the diseased parts, was not cause, but simply concomitant—the same argument which had been used for years before by some dermatologists to prove that the parasite in parasitic skin diseases was not cause, but only an outcome of a favorable nidus.

At the present time but few deny the infectiousness of tubercle. Thirty years ago there was still much argument as to the unity of tubercle and of varied tubercular affections, and much question existed as to the relationship between miliary tubercle and tubercular pneumonia, or, as it was called, tubercular infiltration. Anatomical differences were drawn, by means of which we should discriminate. How simple it seems to the student of to-day, as contrasted with that one of thirty years ago. The attempt was made by this and that reason to make plain what the professor himself was not assured of. Moreover, this discovery of Koch's has, by assigning a definite cause for the

tubercular affections, drawn attention to the necessity of preventive measures in dealing with the excreta which contain the bacilli. The prevalence of phthisis is so great, the apathy as regards careful attention to this matter on the part of some of our profession is still so considerable, and the numbers of those ill adapted to resist the inroads of so prevailing and insidious an enemy, owing to inherited or acquired weak frames, is so great, that we do not wonder, notwithstanding our knowledge of the bacillus is thirteen years old, at the death-rate from this cause. The writer calculated that there must be nearly fifteen thousand cases of the disease in New York city, and that a very considerable portion of these are peripatetic. Yet in preparing a paper on overcrowding, for the County Association, two years ago, he was gratified to find that both in New York and in Philadelphia the number of deaths from phthisis had diminished instead of increasing during the last ten years, as contrasted with those gone before. Nor could this diminution be ascribed satisfactorily to other causes. We hope to see a careful statistical table on this subject at no distant date, which shall comprise most of the large cities, in order that we may be better able to judge as to this improvement. And it is hoped by the writer that each member of the Academy will do all in his power to teach those suffering with the malady, or their friends, the importance of destroying the secretion from the affected parts, and thus aid the efforts made in this direction by the board of health. We do not expect to see the disease completely eradicated, but we feel that with medical insistance and the increase of popular knowledge in this regard, that we shall witness a much more considerable check to the ravages of this fell destroyer.

We cannot believe that the density of population permitted in New York city, twenty or twenty-four families on a twenty-five by one hundred foot lot, betokens medical progress, nor does it point to as successful an issue in the

struggle with tuberculosis and other infectious diseases as may be the case in less crowded places, providing that equally vigorous measures are adopted looking to the destruction of the bacillus. The fifteen, twenty, and more story high office buildings are also fraught with danger in the same direction. So many people are brought together in confined areas that one tubercular invalid, if careless, has much greater chance of spreading the disease. The papers point at these lofty structures, attempting to compel tribute from the sky, with civic pride, but we feel that they are an added source of detriment to health and cannot be considered as medical progress.

Koch may justly be proud of his position in aiding in the progress of medicine. A practising country physician outside of the university, studying carefully with Cohn microscopic botany, he discovered first the link which was sought to complete the life-history of the bacillus of anthrax, a matter which was needed to throw light upon its method of entrance into the system. This was the existence of "dauer Sporen." Then he worked out much about septicæmia, and later cholera as well as tuberculosis yielded up to his search the cause of their being. Moreover, he, more than any other, taught the true method of bacteriological research. Many names admit of honor in this regard. Pasteur, his predecessor, for his long work in the same direction; Eberth, discoverer of typhoid germ; Fehleisen, of the erysipelas coccus; Laveran, of the malarial plasmodium; Obermeer, of the relapsing fever spirachele; Nicolaier, of the tetanus bacillus; Hansen, of leprosy bacillus; Loeffler and Klebs, of the diphtheria bacillus; Fränkel, of the pneumococcus; and Friedländer, of the first variety superseded in great measure by Fränkel. Moreover, a variety of germs capable of exciting suppura tion have been made out by different observers, Ogston, Rosenbach, Passet, and others. Finally, it has been reserved for Kitasato, the Japanese bacteriologist who had

worked with Koch and who had with Nicolaier studied the tetanus bacillus, to work out the bacillus of the plague. He went to China to engage in those studies, which resulted in the discovery, conducting them according to the rules laid down by Koch. This certainly betokens medical progress, and how gratified would Haygarth be, were he living, to know that his argument was correct, though the finding of the organism had occurred over one hundred years after his utterance. More surprised he would be to know that the discoverer was a Japanese, for the plague organism, while Pfieffer had unravelled the secret of influenza in finding a bacillus as cause.

If, without going into detail with each of these, we ask whence the gain from this knowledge, we may well answer, much in every way. The discovery of the cholera spirillum has put in the hands of sanitary authorities a means of distinguishing the disease at an early period, and of taking precautions in consequence of certain knowledge that formerly would have required time and the development of additional cases. In this way, aside from the value attaching to knowledge in the abstract, it has proved of immense importance and will gain in this respect, should the immunization experiments meet continued success.

With reference to typhoid fever we are enabled to know more accurately the conditions favorable to the life of its causative organism, and thus to increase our means of preventing its spread. This knowledge, alas! has robbed us of some of the satisfaction with which we eat raw oysters in the fall of the year, when typhoid germs are being discharged from sewers whose openings in rivers may be near the floats on which the oyster is being fattened or freshened. Medical progress may demand that oysters shall be freshened only in such places as shall be free from liability to typhoid contamination. Previous to the discovery of this organism much of what we now know had been

adduced by careful and painstaking study of the method of origin of the disease; and it has served to direct attention away from sewer-pipes, defective traps, etc., to what was eaten and more particularly drunk, water and milk as the main sources of etiological importance. As far as the writer can see, our knowledge of the pneumococcus is as yet of scientific interest more particularly, though it serves to prove the right of those who, as my former distinguished and revered colleague, the late Professor Austin Flint, claimed pneumonia as an infectious disease prior to the discovery of an organism—and also for possible antitoxin production. The proof of the existence of germs causative of suppuration has in part helped to revolutionize surgery. We say in part, because prior to the proof such far-seeing men as Lister had reasoned out the probability, and laid down rules for the prevention of their entrance in operations, etc. Rules which are now discarded, because the surgeon has learned the value of cleanliness. Cleanliness is next to godliness is the motto, but alas! in bygone times the lack of cleanliness on the part of the surgeon led many a poor soul to learn the mysteries of another world. The writer dreads to think of the number who have succumbed in this city, prior to the era of aseptic and antiseptic surgery.

The treatment of internal diseases has not apparently made so considerable an advance as has the surgical. This is entirely owing to the fact that asepsis has allowed the surgeon to achieve results impossible previously. Yet, when we come to consider that the physician aims at two entirely different results as the issue of his increased knowledge of the etiology and course of disease, we will be struck by the progress which has occurred. He aims, in the first place, to prevent the spread of contagious and infectious disorders, and through foresight, so far as may be, to forestall the influence of deteriorating conditions in individual cases. On the other hand, his duty is to carry

the individual patient through his sickness, whatever it may be.

Though much of the work of protecting the community has been placed upon boards of health, yet it must be recognized that these owe their origin, development, and efficient activity to medical men. A large measure of advice has still to be given by the practitioner to the families under his care, as to the proper methods of procedure against contagion. We have not been able as yet to secure so effective a protection against the other dangerous contagious disorders as vaccination has achieved against small-pox. In this city, during the last thirty years, a study of the statistics will show a remarkable gain in this regard by the establishment of a permanent corps of vaccinators under the control of the board of health. Previously to 1875, when an epidemic of small-pox occurred, then a corps of physicians was assigned to the work of gratuitous vaccination among the poor of the city. Since that date a permanent corps has been maintained, and it can be proven by the results to be the only wise course in a large city. The epidemic of 1874 died out in 1876, and while since then outbreaks occur from time to time, when enough unprotected people exist to form a chain after the exposure of the first to the exciting cause, yet these have not attained the magnitude formerly possible. A careful study of the statistics of small-pox in New York city will convince any reasonable person, not alone of the immense value of vaccination, but also that a permanent corps of vaccinators employed by the city is as essential for the control of the disease. Fortunately, the vaccination law is not compulsory in this city, and hence the anti-vaccination cranks have never been able to reach any considerable number nor obtain any marked influence. They are like the smut on the corn in a field with a large crop, here and there a dark spot, in the wonderful progress of prevention in this disease achieved in this city during the past twenty

years. But this era has been marked by the attempt to control other diseases by a protective influence not like vaccination. Pasteur, who had obtained successful results in dealing with animals by the use of attenuated virus, succeeded in his attempts to find a means of immunizing animals against the hydrophobia by the use of attenuated poison derived from the desiccated spinal cord of rabbits. He also applied this method to human beings, with a result which has given him world-wide fame. Insomuch as in both vaccination and in inoculation for small-pox the germ itself is introduced, grows, and by producing on the one hand vaccinia and on the other a mild attack of small-pox, we may question whether in these protective vaccinations against hydrophobia, with gradually increasing strength of virus, it is not a gradual toxin introduction rather than of the germ of hydrophobia which has not so far been isolated.

Since this time numerous workers have endeavored to solve the question of immunity. The results of vaccination, the fact that human beings and animals recover from infectious diseases, and that one attack of some infectious diseases, protects more or less for a lifetime against subsequent exposures, and in other diseases against a speedy return, led to the search for the method of cure and of a means of immunization against diseases of this nature. It was reserved for Behring to study out and bring to public notice the demonstration that the blood serum contained in tetanus and in diphtheria a substance which has been termed antitoxin, which in each of these diseases had the property to so far antagonize the toxin of disease as if injected previously, with or very shortly after the toxin in sufficient quantity and strength, to prevent the poisoning of the system by the toxin. Kitasato worked with him in the study of the tetanus immunity. Thus was laid the foundation for the serum therapy of diphtheria and of tetanus. In the case of diphtheria, because of the fre-

quent occurrence of the disease, because of its severity and of the number of deaths due to it, this method of treatment has acquired world-wide attention. When we consider the number of deaths from diphtheria, including croup, occurring in New York each year, this city has a vital interest in the matter. The board of health, at the suggestion of Dr. Biggs, and with the coöperation of Dr. Prudden and the work of Dr. W. H. Park, his assistant, began and continues the production of this antitoxin gratuitously for the poor. At the meeting of this Academy on April 3d of this year the subject was presented to the members, and the results then achieved at the Willard Parker Hospital and in the city detailed. Such arguments as could be urged in opposition were presented by Dr. Winters. Since that time the use of antitoxin has continued. The statistics of the death rate from diphtheria and croup in Paris prior to and since the use of antitoxin serum, which is there prepared by Roux, who, after Behring, principally drew the attention of the profession to its value, are so strikingly favorable that, till disproved, no other should be needed. So again, the statistics on the same subject furnished by institutions and physicians in Germany in the great majority are arguments for its use. A very able paper by Professor William H. Welch, of Johns Hopkins University, embodies his views, and is based upon a careful study of seven thousand cases treated by this means. The result is decidedly confirmatory of its merits. Moreover, a number of physicians, in whose judgment and ability the speaker has confidence, have stated to him its favorable effect in extremely severe individual cases of the disease. The papers presented at the meeting of the Academy on April 3d, are arguments for its employment. More recent statistics than these in journals of recent date confirm me as an advocate for its employment. The occurrence of forty-three deaths from diphtheria and croup in this city

during the week ending November 9th, makes the speaker fear that this method is not receiving a prompt and fair trial. Moreover, previous to this date the deaths from the disease have not decreased as have those in Paris.

We would not be understood as stating that there are never any disagreeable sides to this treatment. These the advocates of the method admit, but the speaker does not believe that all the evils which follow the use of antitoxin are of necessity due to the antitoxin. When we consider that the horses used for the production of antitoxin are of a very different constitution, though young and free from disease so far as can be made out, yet the fact that some serums far more than others are attended by urticaria, and by joint pain or rheumatic-like attacks, suggests the idea that if one set of observers finds results at variance with those of the rest or many of the rest, we have a right to conclude that some of the differences are dependent upon the horse from which the serum has been obtained. The possibility that some of the horses had rheumatism is suggested. Neither should such distresses as are to be fairly attributed to it be allowed to outweigh the advantages, when employed early.

Tetanus is fortunately a comparatively rare disease, so that the same interest does not attach to the use of the serum therapy for the possible cure of this disease. Yet the results obtained by animal experimentation stand on the same footing of success as is the case with the diphtheria antitoxin. However, there is the difference that the amount required is larger and greater the longer the symptoms have existed. The cases so far treated in tetanus give a hope that, early and promptly employed, success may at times be obtained to a greater degree than would otherwise be the case. Yet it will require a very careful gathering of statistics to inform us of the absolute value of the remedy.

Experiments in animals have also been made with the

organism and toxin of cholera, and recently experiments made in India seem to show that Hafkeine has succeeded in proving a probability that he has immunized those exposed against this disease.

Claims are now being made for the success of this method for other infectious diseases—pneumonia, typhoid —and more recently for the pus-exciting organisms. There is undoubtedly much of promise in this field, but a liability also to allow our enthusiasm, due to the success in one or more directions, to lead us astray in claiming too much or against a process for which it is not calculated to prove serviceable.

For the last few years, associated with the struggle to prove that carcinoma and sarcoma are not simply outcomes of the bodily development, as a result of unused embryonal tissue stored up in some nook of the body, but are due to the influence of a parasite, animalicular or vegetable, there has gone on an attempt to check its growth by the use of toxins of erysipelas, etc., or the serum of animals treated with such toxin. Our colleague, Dr. Coley, has been an enthusiastic worker in this direction, and feels confident of some success from his work. The writer has not personally been able to witness a case which has turned out successfully, though he has had them subjected to the trial. In view of the fact that successes have occurred, it seems that we will be obliged to assume in these cases a difference of structure or of etiological substratum, such as does not reveal itself to the microscopist. The writer trusts that either by the means now used, or by the intervention of some additional agencies, we may be enabled to add to the progress of medicine in other directions that of having achieved a victory over the malignant tumors.

The attempt to cure consumption (tuberculosis) has been oft repeated, and in view of the prevalence of the disease, which kills 5,000 people annually in round num-

bers in New York city alone, no wonder is excited by the frequent appearance of a new method for which great promise is made. Thus, of late years especially, the hot-air inhalation, the Bourgeron carbonic-acid and sulphuretted-hydrogen injection, and, last, the tuberculin plan, have come successively before the medical and lay public with great pretensions. The two former quickly sank to their proper level of inefficacy; but the latter came heralded by the name of one who has done so much for the medical progress of the last twenty years that it was not alone the hope for a cure of this dreaded disease, but interest that the discoverer of the germ of origin of the disease might also be the finder of the means of its destruction, that gave intensification to the announcement. We must regret the eager striving to be the first to use this new remedy, product of difficult and prolonged research on the part of its renowned projector, which rather marred its introduction. At present it is but little used, though there are those who, like my friend, Dr. Trudeau, are making continued tests in those who desire to make trial of it; and I am told that Professor Koch is himself still at work endeavoring to secure some method by which it may be employed with success. The speaker, and I have no doubt all his auditors, wish him God-speed in his efforts. As it is, we find no better method for the cure so far than the effort to develop the patient's internal powers of resistance by suitable climate, air as free as possible from the germs of suppuration, improved nutrition, and appropriate medication in such better atmosphere where as much as possible an out-door life may be led.

A visit to the sanitarium at Saranac Lake would well repay each of my auditors. The cottage plan has been adopted except in the oldest structures, each holding four inmates, with a common living-room and a separate bed-room. The outlook is over an unbroken forest, save a hill, to protect from the worst winds, situated behind.

Here eighty patients receive treatment and board for a price which has to be supplemented by the gifts of the charitably disposed. Moreover, in his laboratory there Dr. Trudeau continues his experimentations upon tuberculosis. This laboratory is a surprise to those who have not seen it before. It is the gift of the late Mr. George Cooper to replace one which had been burned.

Not alone has the present era witnessed the introduction of these new agencies, of which our fathers in medicine did not even dream, and at which they would have stood aghast, but it has largely increased the number of agencies to be used in disease. It has become a habit to decry the use of the coal-tar series in the treatment of febrile disorders, but the writer has found them, judiciously used in appropriate cases, of service, and he does not hesitate to say that he still sanctions their employment under these conditions. How can we admit the claim of those who formerly saw great results from their use, and now say that they have no value? Many undoubtedly have employed excessive doses, and have attempted their employment in such doses unguarded, in unsuitable cases, with at times unfavorable results, but this is no argument against the statement made above, nor to their own previous claims. No one method of treatment can be at all times applicable to all cases. While this is true, the very general trend of medical work in fevers has been to substitute the use of cold water in one or another way for the medicinal agencies in bringing about a reduction of temperature. First, sponging and use of the coil, then the pack, and now more than formerly the use of the tub-bath is the plan adopted. The use of the tub-bath is not alone for reduction of temperature, which does not of necessity occur, but as much for its effect on the circulation, nervous system, and with a view to prevent the ill effects brought about by the stagnation of the circulation in the lungs and limbs. But in this connection how much better would the ounce of preven-

tion be than the pound of cure. Certainly we must urge that boards of health, so far as is possible, shall have the power to inspect the water-supply, of all communities, and also of every kind, of each dairy furnishing milk for consumption in their town or cities—and in case there is a suspicion that the conditions are liable to be dangerous as regards any water-supply, whether used for washing milk-cans or otherwise, that they prohibit such dairy from supplying milk so long as such danger exists. If such inspection had been effective with a law of the above nature, the outbreaks of typhoid fever in Stamford and New Milford, Conn., and in Montclair, N. J., and Watertown, N. Y., would have been prevented. These were all due to water-contamination of milk, said only to have come about by washing cans.

It would require too much of time and impose on your indulgence should I go over the list of new remedies of a strictly medicinal nature which have come into use, as strophanthus, iodoform, sulphonal, trional, ichthyol, cocaine, etc., not to speak of the change which has in particular resulted from the success of the thyroid feeding to make up for the deficiency of the normal secretion from the gland, as bone marrow in anæmia, suprarenal capsules in Addison's disease, the use of extracts of certain organs in diseases of those organs, further than to say that our credulity is largely borrowed on at times by the reports which are in circulation and also in print. If some of these remedies are so efficacious, why not resort to the raw egg, from which certainly the whole system of the chicken is built up—bones, blood, brain, spinal cord, muscles, etc.? The old-fashioned egg-nog had a virtue evidently based upon a theory of which our grandmothers were ignorant, when they would administer this to a convalescent. Moreover, those who ascribe such dangerous bilious-producing properties to the yolk of egg, in many cases the result of this syllogism, bile is yellow, yolk of

egg is yellow, hence yolk of egg is liable to induce biliousness, must beware how they slander so powerful a food.

The growth of specialism has undoubtedly tended to medical progress, and this has been largely an affair in certain directions during these past thirty years. There are some parts of the body which more than others call for special treatment, because the dexterity acquired by constant practice makes one largely excel another who is more occasional in his work. The eye, the ear, the nose, and throat claim attention, and gynecology and andrology have come to be branches of special study. Moreover, the field of internal medicine is split up into subdivisions under the control of specialists and especialists; hence we have neurologists, cardiologists, pneumologists, gastrologists, enterologists, hepatologists, hæmatologists; and not to lengthen this list of words, which are deserved if not applied, those who devote themselves to the latter end.

The writer does not believe, except in some of the fields to a certain extent, that a man can be a first-rate specialist who has not had a broad enough foundation of knowledge to appreciate the relationship between diseased states of organs and the secondary results of such states, both on the general economy and upon other organs, as well as the reverse of this. And it is also true that many specialists have been obliged to extend the sphere of their work because of this interdependence of parts. The aurist and oculist find that the brain will claim attention, and that the condition of the general health is of great importance. Moreover, it is the writer's conviction that by considering too exclusively the disorders of one part all morbid phenomena are liable to be referred to errors of this part. The speaker would not advocate giving up specialties, but he would advise young medical men not to start as specialists until they have acquired a broad general knowledge of the effects of disease upon the body.

Along with the growth of specialism has come with the progress of our time the medical expert. He existed before, but not to the extent that he has of late years. The medical profession is at times surprised to find who are experts upon certain questions, and also to find what diametrically opposed opinions experts will express in regard to the same condition of affairs. One expert will swear that a man was insane because at such and such a time he did so and so, acted in such a way, spoke such words, saw perhaps hallucinations, or what were regarded as these; while another alienist or neurologist will explain these as compatible with sanity. In a trial some ten years ago three experts called for the defence in a case of nervous trouble due to an injury to the spinal column resulting from a fall, each swore that the man had a complaint different from what his associate had considered it, and all swore that he did not have what the speaker and Professor Sayre swore to, viz., that he had Pott's disease and subsequent pachymeningitis. Moreover, one of these medical expert neurologists wrote an article in which he took us to task for the character of our testimony. An autopsy in the case two years after the trial showed Pott's disease.

The patient won his suit and recovered $25,000 damages. It does not, however, do for the lawyers to rail at us in view of the court after court to which cases are taken and retaken, the judgments upset and confirmed, to be upset on appeal, and the court of highest resort not infrequently divided in its decision, and at times even practically deciding against the judicial soundness of an opinion previously promulgated. We may well say to the lawyer: Don't point your hoary finger at us, but correct your own infirmities which are many, including the tendency to ever-recurring delay and postponement, the *bête noire* of the medical witness.

Whether in the future some plan can be devised which will avoid this conflict of testimony, by arranging for the

selection of several physicians so chosen that they shall represent the best attainable medical intelligence upon the condition of the person about which there is a dispute, may be open to question, owing to the laws relating to trial by jury. There is, however, not the least doubt about the desirability of such a change. A medical expert who goes on the stand for a particular side, no matter how firm his intention of telling the truth, the whole truth, and nothing but the truth, is very apt to be biased in favor of the view advocated by his attorney, and moreover, to be, in a measure, unwittingly antagonistic to the lawyer of the opposite side, who is to cross-examine him. Expecting an attack upon the opinions he has expressed, he mentally endeavors to foresee the method of meeting such attack.

In view of the facts which I have so briefly touched upon, showing the progress of medicine in so many directions, requiring of the busy practitioner, as well as of the medical student, increased effort to keep up with the ever-increasing mass of knowledge, and of which he may readily find the information in the books and journals belonging to the Academy of Medicine; it does seem as if this Academy might be able to give to its fellows, without encroaching in the least upon the provinces of the laboratories attached to our medical colleges— a laboratory in which there might be the means of investigation of the different pathological discharges, and in which there should be also exemplifications of the recent advances in scientific research. By reading, he is told what someone else has seen and done. I would have him part owner of property in which he could witness the thing itself. How extensive such a laboratory should be would become a matter for discussion after it had been decided to have a laboratory. The writer believes that upon a proper presentation of the advantages likely to accrue, not only to the physician himself but also to his patients, from his increased efficiency, funds could be secured from those of the members inter-

ested in the best welfare of the Academy, and also from those friends of the Academy who are neither members nor physicians—citizens of New York desirous that its medical institutions shall be the best equipped for their work of any in the land or world.

Objection may be urged against this plan upon the ground that a debt exists at the present time, and that funds are needed for the library, which should be extinguished before any new enterprise is undertaken. Then let us endeavor to extinguish this debt, to secure our laboratory, if the members decide that they wish one, and to obtain as large a library fund as possible, even a library and laboratory building, if our friends will avail themselves of the opportunity of bestowing upon a hard-worked profession the means for its truest recreation.

Stated Meeting, December 5th, 1895.

A STUDY OF THE INFECTIOUSNESS OF THE DUST IN THE ADIRONDACK COTTAGE SANITARIUM.

By IRWIN H. HANCE, M. D.

Associated with our increasing knowledge of germ life and the practical application of bacteriology toward solving the causes of disease and tracing back to its fountain-head the source of any infection, their exists among the laity and also some physicians an intense fear, sometimes amounting to mania, that they are continually exposing themselves to an invisible danger which is lurking about them in the food they eat, the water they drink, and the air they breathe. The most dreaded nightmare of them all is the fear of becoming infected with the germs of tuberculosis. No one can deny the presence of this danger in a great many places; but oftentimes in the places where popular opinion considers it greatest it is actually less to be dreaded than in other localities where with the external appearances of refinement and cleanliness there exist the most favorable conditions for the permanent lodgment of infectious material in the dust.

This fear of infection from tubercular germs has become so general and reached such a climax that the poor tubercular patient whose mental and physical suffering is already extreme must carry around with him an added burden of anxiety and care, since people look upon him as a dangerous character in the home or community. It is within the writer's experience that parents have refused to have their children return home lest some other member of

the family might be infected; and men have found it difficult to hire flats in New York city or Brooklyn when they wished to make a home for a dying wife or child. In one instance a physician wrote to have his former patient kept in Saranac Lake until she died, as he thought that was the only way of avoiding possible infection for the other members of her family; in the end she was taken home to die in a hospital.

Such heartless actions are due to a great extent to the ignorance of people concerning the real dangers of infection, and their lack of knowledge as to how they can be avoided. In their blind ignorance they think to avoid an apparent danger, little dreaming how much more frequent and dangerous are the daily exposures to the same diseases in their social intercourse, travels, and amusements.

For these reasons it has seemed a fitting time to make some experiments on this subject to determine how great the danger of infection is in a large community of consumptives where all sanitary measures are enforced as regards the care and disinfection of sputa.

Two facts have been positively affirmed by previous experiments: 1. Buildings for consumptives, prisons, private dwellings, and public conveyances do become infected with tubercular germs. 2. A tuberculous patient is absolutely free from the danger of infecting others by contact; it is the product of their excretions which is the dangerous factor. Destroy these and the house or home of the consumptive is no longer dangerous.

All investigators have established the first of these two statements, none more thoroughly than Cornet,[1] reference to whose work will be made later in this paper.

The second statement has also been repeatedly proven; but among the laity and also some physicians it is so dimly

[1]Cornet: Die Verbreitung der Tuberkelbacillen ausserhalb des Körpers. Zeitschrift f. Hygiene, 1888–v–191.

comprehended that time will not be lost in quoting some of the investigators and their experiments.

Two Italian investigators, Celli and Guarnieri, found the expired air of tubercular patients free from tubercle bacilli; and also that air blown by a bellows over and through sputum very rich in bacilli remained free from bacilli.

Fr. Müller,[1] Sormain and Brugnatelli,[2] Charrin and Karth,[3] Cadeac and Malet,[4] all found the expired air non-infectious.

For two months Tappeiner[5] had a woman with advanced phthisis cough through an opening into a wooden box in which were two guinea pigs; at the end of that time they were killed and found sound.

Cornet[6] states that the expired air "never and under no conditions contains tubercle bacilli or their spores."

Since, then, personal contact with a tubercular patient is free from danger, whence is the source of the infectious material? For all practical purposes it is contained in the sputum, which becomes dangerous and capable of infecting when dried and pulverized. In its moist state it is harmless; therefore, if tubercular patients are careless and dirty about their sputum they are dangerous, not alone to themselves, but to all who come in contact with them.

Knowing these facts, and taking pains to make them clear to the patients in the sanitarium, it may well be said,

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[1] Fr. Müller: Ueber die diagnostische Bedeutung der Tuberkelbacillen. Würzburg, 1883. Verhandlungen der med. phys. Gesellschaft zu Würzburg. N. F. Bd. viii.

[2] Sormain and Brugnatelli: Studi sperimentali sub bacillo della tuberculosi, 1883.

[3] Charrin and Karth: Virulence de la tuberculose suivant les humeurs et les tissues des tuberculeaux. Revue de Mèd., 1885-v-659.

[4] Cadeac and Malet: Étude experimentale de la transmission de la tuberculose par l'air expiré et l'atmosphére. Revue de Méd., 1887-vii-545.

[5] Tappeiner: Zur Frage der Contagiosität der Tuberkeln. Archiv. für Medicin, 1881-xxix-595.

[6] Cornet: Loco citato.

after five years of personal observation, that the rules concerning the care and disinfection of sputum have been since its inception and are to-day most carefully carried out there. Occasionally a very sick or careless patient does transgress the rules, but this occurs very seldom for such a large institution.

In order to test the efficacy of the system, a complete examination of the group of buildings was made, some of which have been occupied eleven years by consumptives. In every instance dust from the darkest and most likely to be infected spots was taken; for if infected, the management was most desirous of knowing it.

The experiments were done at Dr. Trudeau's request and under his supervision at the Saranac Laboratory for the Study of Tuberculosis. They were arranged in two groups, the details of which were as follows: First group of four buildings consisted of the main building (parlor, sitting-room, and public library); the infirmary, where all the acutely sick are sent; the "Red Cottage," which was the oldest on the grounds, and the "Penfold," the most recently built cottage. One square yard of dust from each of these buildings was collected and inoculated into ten guinea pigs. Second group consisted of thirteen cottages, from each of which half a square yard of dust was taken and inoculated into three guinea pigs; thus the second batch of pigs received proportionately a larger quantity of dust than the first.

The technique was as follows: Sterilized cotton swabs moistened with sterilized water were used to collect the dust from the walls, backs of pictures, and darkest corners of the rooms; 1 to 2 c.c. more of sterilized water was added to cleanse the cotton of the dust. The swabs were then squeezed in a piece of coarse sterilized gauze, and an equal share of the whole dust, suspended in water, inoculated into each guinea pig; all inoculations were made into the abdominal walls, extraperitoneal. Save for any infec-

tions in the dust, all other sources of contamination were excluded by thorough antiseptic precautions.

The animals were kept from one to three months and then killed. Any enlarged glands or suspicious areas were carefully examined for tubercle bacilli, and the diagnosis of tuberculosis only made when these were found. In the painstaking details of these prolonged experiments, the writer wishes to express the appreciation of the assistance given him by Dr. S. W. Heweston, who aided him materially in the work.

The results are shown in the following table, which is arranged under the same heading as in Cornet's work in order to compare the results:

PLACES.	Number of Animals.	Of which died of		Died of infectious diseases.		Remained sound.	Amount of dust used.
		Tuberculosis.	Other infectious diseases.	All told.	In percentage to inoculated animals.		
Main building: Parlor, sitting-room and library	10	.....	1	1	10	9	Sq. yard. 1
Infirmary	10	.....	3	3	30	7	1
Red cottage	10	5	.....	5	50	5	1
Penfold	10	.....	.....	.....	.....	10	1
Loomis	3	.....	.....	.....	.....	3	½
McAlpin	3	.....	.....	.....	.....	3	½
First Stokes	3	.....	.....	.....	.....	3	½
Second Stokes	3	.....	.....	.....	.....	3	½
Green	4	.....	.....	.....	.....	4	½
Lea	3	.....	.....	.....	.....	3	½
Spruce	3	.....	.....	.....	.....	3	½
Trudeau	3	.....	.....	.....	.....	3	½
Pine	3	.....	.....	.....	.....	3	½
Sunshine	3	.....	.....	.....	.....	3	½
Schiff	3	.....	.....	.....	.....	3	½
Dodge	3	.....	.....	.....	.....	3	½
Strauss	3	.....	.....	.....	.....	3	½
Out-building	1	.....	.....	.....	.....	1	¼
Thus of	81	5	4	9	11.11	72	

In all eighty-one pigs were inoculated with from 2 to 3 c.c. of sterilized water with the dust in suspension; of these only 4 or 4.9 per cent. died of other infectious diseases on the third to sixth day and five of tuberculosis. These five constituted just one half of the number of pigs inoculated with dust from the "Red Cottage," a small cottage holding two patients, always occupied by the sickest men, one of whom had been complained of by his roommate for spitting around the cottage. The five pigs lived sixty days, were then killed, and although tuberculosis was present in the omentum, spleen, liver, and lungs, they were well nourished and strong, apparently showing a diminished virulency of the infecting germs.

The infection of this cottage demonstrates two things: first, how easily a patient by carelessness and disobedience of rules (probably due to his very weak, sickly condition) may render a cottage dangerous to himself as well as to others; second, that the technique of the experiment was faultless.

It brings out, however, in stronger contrast, the successful results of the experiment as a whole, since sixteen buildings out of seventeen, inhabited by consumptives for so long a period as ten years, were absolutely free from infectious material. This is the more striking when we consider the great vitality and virulence of the tubercle bacilli, Stone[1] having proven by inoculation of rabbits that dried sputum after the lapse of three years was capable of inducing tuberculosis.

With this array of negative results a most conclusive proof is given that a body of consumptives need not infect the houses they occupy when their excretions are destroyed. To attain this end it requires that each new patient should be carefully instructed concerning the disposal of his sputum, and close supervision of them all be maintained.

Then each patient feels that he endangers himself as

[1] A. K. Stone: American Journal of the Medical Sciences, 1891–CI–275.

well as the others by not obeying the rules; if anyone becomes careless in the matter it is quickly reported to the medical authority by one or more members of the sanitarium, everyone appreciating fully the grave consequences of possibly infecting the cottages or public rooms.

The methods used are to burn all cuspidors daily and the Japanese napkins as soon after using as possible; never to expectorate except into the large sanitary cuspidors when about the main buildings, or the small individual hand cuspidors. Paper napkins are used in the infirmary in hemorrhage cases or where patients are too feeble to get up on their elbows so as to spit into the cuspidor; these are used but once, then placed in a pasteboard receptacle and the whole thing burned several times a day. During the greater portion of the year fires are burning in every cottage, and the patients burn everything in the stove or fireplace.

In summer a barrel with some moist sawdust in the bottom is placed in an out-building, and the cuspidors and napkins are carried to this barrel, which is saturated with kerosene and burned three times a week. One fourth of a square yard of dust from this building was inoculated into one guinea pig, which remained healthy. A properly constructed crematory is much needed, but too costly at present to build. The cuspidors used are made by Seabury & Johnson, and of two sizes; one, the hand cuspidor, for individual use, and the other, a much larger size, for the piazzas and public rooms. The latter are placed in covered wooden boxes about four feet from the floor; these are easily taken down and cleansed. By this arrangement patients cannot spit at a cuspidor from a distance, and the mass of sputum is not blown over the edges of the cuspidor by the wind, as happened at first when uncovered cuspidors were placed upon the floors.

It is, perhaps, too much to suppose that all particles of infectious materials are thus destroyed; where seventy to

eighty small hand cuspidors are in daily use there may be some slight soiling of a stand or the linen covers on which the cuspidors are placed; also in coughing some small masses of sputum may be violently expelled. These sources of contamination must be small, and are fraught with less danger by frequently washing the tin frames for the cuspidors, and the cottage plan of housing patients, thereby avoiding the evils of overcrowding.

Besides the destruction of the sputum, other factors equally important have served as auxiliary means of keeping the cottages free from infection, not alone with tubercle bacilli, but also other germs; these are the construction of the cottages, which are built so as to insure thorough ventilation; the immense volume of air space allotted to each patient—this is secured by large openings from the various bedrooms into the sitting-room, which communicates with the outside air by transoms opening above the piazza roofs; the smooth, hardwood wainscoating, seven to eight feet high, around the whole cottage, permitting of thorough cleansing with soap and water; the absence of wall-papers, the upper walls and ceilings being heavily sized and then painted, rendering these also easily cleansed; the plain, simple furnishings—iron hospital beds, rugs, and hardwood floors, and as few tapestries as possible; lastly, the large area of window surface, allowing the sun's rays and strong light free access to all parts of the cottage. Added clinical proof of the non-infectious character of the dust may be deduced from the fact, that not one of the twenty to twenty-five attendants has ever developed tuberculosis; and also that no patient who was admitted suffering from pulmonary disease without the bacilli being present ever subsequently developed them.

How do these results compare with those of other experimenters?

Heron[1] inoculated one hundred guinea pigs with dust

[1] G. A. Heron: The Relation of Dust in Hospitals to Tuberculous Infection. Lancet, 1894–I–371.

taken from various sources in the City of London Hospital for Diseases of the Chest. In the greater number of experiments "a piece of dust was introduced into the subperitoneal tissue;" in some a solution of dust in sterilized water was injected into the pigs. Twenty-six pigs, twenty-six per cent., died of "either intense inflammation spreading from site of inoculation or septicæmia." Of the remaining seventy-four, two pigs (both inoculated with dust in solution), or 2.7 per cent., died of tuberculosis. The dust in each case came from "the tower of the hospital which acts as up-cast shaft," and was dark and unventilated. It would seem as though the implantation of a piece of dust was too small to place too great reliance on negative results.

Cornet,[1] seven years ago, gave us the following table:

PLACES.	Number of animals inoculated.	Of these died of		Died of infectious diseases.		Remained sound.
		Tuberculosis.	Other infectious diseases.	All told.	In percentage to inoculated animals.	
In seven hospitals.....	94	20	52	72	76.6	22
In three insane asylums..	33	3	16	19	57.5	14
In two prisons.....	14	.....	6	6	42	8
Inhalation rooms.....	4	2	.....	2	50	2
Dwellings of private tubercular patients.....	170	34	91	125	73.5	25
Polyclinic, orphan asylum, etc.....	28	.....	14	14	50	14
Surgical wards.....	8	.....	1	1	12.5	7
Streets and hygienic institute.....	41	.....	16	16	39	25
Streets alone.....	.....	.....	.....	.....	55	.....
Thus of.....	392	59	196	255	65.5	137

In the hospitals 47.6 per cent. of the pigs not dying of acute infection developed tuberculosis; in private dwell-

[1] Cornet: loco citato.

ings, 43.6 per cent.; in insane asylums, 17.6 per cent.

M. Kirchner,[1] in examining the garrison lazaret of one of the army stations, experimented on forty-two pigs by inoculation with dust suspended in bouillon or implantation of dust-infected sponge in abdomen. Twenty-six, or 61.9 per cent., died from the operation and infection of wound. Sixteen, or 38.1 per cent., remained alive. One of the sixteen developed tuberculosis.

The writer secured the dust from a large city hospital out of the wards occupied by male and female tubercular patients. From male ward three pigs were inoculated with one half square yard of dust. From female ward three pigs were inoculated with one half square yard of dust. From female ward again three pigs were inoculated with little less than one half square yard of dust. Of male ward pigs, one died; of female (first lot), all three died; of second lot one died of an intense cellular inflammation much resembling a malignant œdema. All these died on the first to third day after inoculation. The four living pigs were killed in sixty days, and one of those inoculated with dust from female ward had well advanced tuberculosis; thus, 25 per cent. of the pigs remaining alive developed tuberculosis, while 55.5 per cent. died of acute infection, some of which might have developed tuberculosis. In these wards the beds were carbolized once a week; the floors washed daily and scrubbed twice a week; the walls rekalsomined every six weeks, and Seabury & Johnson's cuspidors used, which were changed twice a day. In the female wards, however, long pieces of cheese-cloth were given to the sickest and weakest patients, which were used as a roll to spit into, and would last a patient from four to eight hours. This, in all probability, was the source of infection.

[1] M. Kirchner: Einige Untersuchungen von Staub auf Tuberkelbacillen. Zeitschrift f. Hyg. u. Inf. Krank, 1895-XIX-153.

A close perusal of these results shows that the careful disinfection of the sputum has been productive of good results, but not yet fully satisfactory, since in each experiment the dust has been infected with tubercle bacilli. Of far more importance, however, is the fact that the dust was infected to such a virulent degree with other germs; to-day we know how much more serious are the cases of tuberculosis with mixed infection than are those of simple tuberculosis; that the latter may quickly be changed into the former when patients must live in an atmosphere laden with infectious germs is scarcely to be wondered at, inasmuch as, according to the above experimenters, 316 out of 543 inoculated pigs (or 58.1 per cent.) were killed by some form of infectious disease, the germs of which were contained in the dust.

Mixed infection is a most important factor in relation to a tubercular patient's condition, whether this arises from without by inhaling the germs of a heavily infected atmosphere, or is the result of an inflammatory process in the lungs. To it may be attributed the sudden and rapid changes for the worse in patients, who were doing well under proper climatic surroundings, after a sojourn of two to four weeks in a large city.

May not the condition of "hospitalism" met with among the internes of the large hospital, and so frequently the precursor of tuberculosis, also be due to the same cause?

A few words concerning the best means of disinfecting sputum. Fire is unquestionably the very best disinfectant, but patented cuspidors are expensive; in place of these any cheap material, old pieces of linen, Japanese napkins, or even pieces of newspaper can be used. If such are made use of *they never should be used but once*, then, after being placed in some suitable receptacle, they, with the receptacle, should all be burned together. In a word, nothing into which a patient has expectorated should ever be handled again, and should not be allowed to dry. M.

Kirchner[1] has shown, by inoculation-experiments, that sterilization for one half hour is a thoroughly efficacious disinfectant.

Dry heat (100° C.), however, must be applied for several hours (Schill and Fisher).[2] The same experimenters give their conclusions concerning the disinfecting powers of various antiseptics as follows: Sublimate solutions cannot be considered suitable for disinfection of masses of tubercular sputum. Absolute alcohol gives indifferent results. Equal parts of a 5 per cent. carbolic acid solution and the amount of daily sputum will destroy with certainty the tubercle bacilli and spores in the sputum.

Delepine and Ransome[3] have shown, by inoculation experiments, that solutions of chlorinated lime, 1 to 10 and 1 to 100, will satisfactorily disinfect the walls of infected rooms, or bed-linen and clothing that has been infected.

It is to be hoped that formaline will prove an efficient disinfectant owing to its easy applicability and its non-corrosive action upon metals. The most powerful and at the same time freely obtainable agents are the sun's rays and diffused daylight. They can be used by everybody, and have been proven to be the most efficient disinfectants known.

It has been the writer's aim to enlighten the public mind concerning the freedom from danger of contact with tubercular patients, where the necessary sanitary precautions are taken to destroy all sputum. This is shown by the positive proof that, with the exception of one cottage where cause for infection was discovered, all the cottages, the infirmary, and public sitting-rooms of a community of

[1] M. Kirchner: Ueber die Nothwendigkeit die beste Art des Sputumsdesinfection bei Lungentuberculose. Zeitschrift f. Hyg. u. Inf. Krank, 1892–XII–249.

[2] Schill and Fisher: Ueber die Desinfection des Auswurfes der Phthisiker. Mittheilungen aus dem Kaiserlichen Gensundheitsamte, 1884–II–133.

[3] Delepine and Ransome: A Report on the Disinfection of Tubercle-infected Houses. British Medical Journal, 1895–I–349.

eighty tubercular patients were found free from tubercular infected dust.

Since Koch's discovery of the tubercle bacillus and the subsequent proof that it alone is the cause of the widespread contagion of tuberculosis, the enforced improved hygienic laws and the dissemination of the knowledge of successful preventive measures have been productive of good results as shown by the mortality tables of various countries, viz.: In New York state the mortality in 1889 was 120 per 1,000, whereas in 1894 it was only 108.46 per 1,000.

Much more can and will be accomplished within the next few years by education of the public and preventive legal measures; along with our efforts, however, to prevent those who are tuberculous from infecting their fellowmen let us at the same time look out for their well-being and try to save the lives of many of them. How better can we accomplish this than by patterning after the sanitarium where sunlight, fresh air, good food, and out-of-door life, with separate cottages for each group of four or five patients, allow them the best chance of recovery?

On the question of comfort, on the question of pure air, free from all germs of disease, on the question of economy, on the question of hygiene, and finally on the whole question of humanitarian principles, how much better off is the lingering consumptive in the open country or in the mountains than he who is cooped up within four walls of a city hospital with a measured cubic foot of air-space, spending his days and nights in an atmosphere artificially ventilated with air that is already contaminated?

DISCUSSION.

DR. E. G. JANEWAY.—Few of us, I am afraid, appreciate the arduousness of the task which is entailed in obtaining such facts as have been presented to us to-night in the brief paper read by Dr. Hance. I was glad to be present to hear the results of his labors, and I do not expect to

communicate anything nearly so valuable. During a recent visit to the Adirondack cottages, the absolute cleanliness, the free ventilation, and the small number of the inmates made a great impression on me; in fact, I would much rather live in some of those cottages than in many of the hotels throughout the country which are largely frequented by patients with pulmonary disease. I should rather have a delicate patient inhabit one of those cottages than to go to some hotel where I would be in doubt about the cleanliness of the room or its previous inmate. In sending a delicate patient away for the winter, this fact should be borne in mind. He may be given a room in some hotel which has just been occupied by a consumptive individual who, perhaps, was careless, expectorating on the floor and walls, and if the room has not been carefully cleansed afterwards, tubercular infection may occur. In sending such patients away, I often advise them not to stop at hotels which are much frequented by pulmonary cases unless they are assured that the utmost precautions are taken regarding cleanliness. Then again, in sending away tubercular patients, we should be careful that they do not become still further infected. The subject of mixed infection is one of much importance.

In the treatment of tubercular patients, the disinfection and destruction of the sputa and other excreta likely to contain the tubercular germs should be constantly borne in mind. Instances daily come under my observation in which the importance of such disinfection has not been impressed upon the patient or his friends. There are many physicians in this city who simply prescribe for a tubercular patient and leave without saying a word about the care of the sputa and other excreta. If the same care was exercised here in New York as is done in the cottages at Saranac, we would see less tuberculosis than we do.

I think the presentation of facts made by Dr. Hance is very timely, and it certainly should induce the medical profession in this city to put forth greater efforts than they have in the past to prevent the spread of tubercular infection. With the running waters, sewers, etc., that we have here, more can be done in this direction than in the country, where all the refuse is thrown on the ground, there to dry up and possibly become a fresh source of infection.

DR. H. M. BIGGS.—We have to-night listened to a val-

uable paper upon a subject which has interested me for many years. I was exceedingly gratified to learn that effective measures to prevent the extension of tuberculosis can be taken in an institution devoted exclusively to consumptives, and it seems to me that the paper helps clear up many questions which have caused a good deal of uncertainty and confusion in the minds of both the profession and the laity. As proof of the statement that there is very slight danger of infection from tuberculosis, cases are constantly being cited where patients were treated for a long time without infecting the attendants or physicians; the hospitals of Great Britain are often cited as examples of this. Those who make this claim have lost sight of the fact that where proper precautions are taken, tubercular patients are entirely free from danger.

Three years ago, when the board of health of this city proposed to class tuberculosis among the infectious diseases, a great outcry was raised against it by many laymen and physicians, who claimed that such patients would then become a source of horror to all their friends, who would avoid them as they would lepers. We have to-day the strongest evidence to show that in cases of tuberculosis where proper precautions are taken to dispose of the sputum, there is no danger of infection, while if such precautions are neglected, the very greatest danger of infection exists. My attention has recently been called to this matter in one of our large city hospitals, and on investigation I found that during the past two years eleven nurses and orderlies have been dismissed from that hospital because they were suffering from tubercular disease; in every instance, these persons were free from the disease, so far as could be determined, when they entered the service. In this particular hospital, the hygienic conditions are exceedingly bad; the wards are overcrowded and the ventilation defective. Besides this, the class of patients are of the poorest and it is difficult to enforce effective measures to destroy the sputum. I think every one who has been connected with any of our large hospitals has noticed the development of tuberculosis among patients who were not previously affected with that disease.

I can fully endorse what Dr. Janeway has said regarding the lack of instruction given patients by their physicians. Numerous instances of this have come under my

observation since the health board has taken means to cleanse the apartments formerly occupied by tuberculous patients. Since this work was taken up, we have inspected about 18,000 cases, about half of these having been deaths from tuberculosis and the remainder living cases, and I can state here that it is the exception rather than the rule to find patients who have been properly instructed by the attending physician in regard to the dangers of infection. These dangers have so frequently been demonstrated that it is nothing less than criminal negligence if the physicians do not carefully instruct their patient in this connection. With the facilities now at the command of the profession, there is no excuse for this lack of precaution. In any suspected case of tuberculosis, the sputum can be sent to the board of health for microscopical examination. By making these examinations in the early stage of the disease, an earlier diagnosis is possible and the patient's chances of recovery are better. I trust that some day the city will establish a sanitarium in the neighborhood of New York where poor patients with tuberculosis could be properly cared for and where the early cases might have at least a chance to recover.

I again wish to express my thanks to Dr. Hance for his admirable paper and the careful work which it represents.

DR. H. P. LOOMIS.—It has been my privilege to visit the sanitarium at Saranac every year since its foundation and I well remember that at that time the idea of a community of consumptives was disapproved of by a large number of the profession, on the ground of the contagiousness of tuberculosis and the danger arising to cases in the incipient stage from too close contact with those further advanced in the disease. The careful experiments recorded by Dr. Hance in his interesting paper to-night give added proof to the fact that the danger of infection from tuberculosis is practically overcome by the careful and conscientious destruction of all sputum. The housing of patients in small cottages containing from three to five persons seems to be an admirable solution of the second difficulty. The lot of the poor consumptive is certainly a hard one, and with the wards of all the hospitals in this city, except one, closed against him, it should be our duty to emphasize the little danger that exists from infection where proper care is exercised.

DR. W. FREUDENTHAL.—I simply wish to mention the fact that at the Montefiore Home quite a number of the nurses have become affected with tuberculosis. This is in accord with Dr. Biggs's experience. While the ventilation there is much better than at Bellevue, and it is not so overcrowded, still the condition of the patients is such that absolute cleanliness is not possible. Whether the dust alone is the source of infection in these cases I do not know; I am inclined to think that there are other factors by which these nurses become infected.

DR. A. H. SMITH.—While this subject is one of transcendent importance in our cities, it is also important in the rural districts, and I trust that the statements embodied in Dr. Hance's paper will be circulated among country practitioners. In many houses in the country no changes in the furniture or decorations are made for generations, and the rooms are kept tightly closed for long periods of time. Under such conditions as these I have seen entire families swept away, one after another, by tuberculosis. Physicians practising in the country should see to it that after the occurrence of a case of tuberculosis, the house should be thoroughly renovated.

Another point to which I desire to call attention is the unsanitary condition of the night-trains, especially those running to various health resorts—Saranac, for instance. The sheets in the sleeping-cars are probably kept clean enough, but what about the blankets? The same blankets are no doubt used night after night. I feel so strongly about this that I never allow one of my patients to travel on the sleepers if it can possibly be avoided.

DR. A. JACOBI.—The remarks which have been made apply not only to tuberculosis, but to all other contagious diseases which are considered by the public much more dangerous than tuberculosis. For instance, I have seen cases of diphtheria in the same suites of rooms of certain hotels in this city year after year, the residents changing, and I feel convinced that the carpets in those rooms were not removed and that the furniture and walls were not disinfected. If this precaution is neglected by the hotel proprietors after the occurrence of diphtheria, which is recognized by everybody as a highly contagious disease, how much more negligence will they display about tuberculosis?

There is another class of houses in this city which I think would prove a very fertile hunting ground for the board of health. I refer to the better class of tenement houses, the so-called flats or apartment houses, which shelter anywhere from six to twenty families each; the stairways are usually covered from top to bottom with carpet, which in many cases is left undisturbed for years. Tubercular patients going in and out of these houses expectorate on the carpet and there the sputum is left to dry and to be a constant source of danger to others.

DR. C. A. LEALE.—I desire to thank Dr. Hance for his excellent paper showing so much care and intelligent labor before reaching his conclusions, which many may have had difficulty in understanding. While visiting the London Hospital for Consumptives, as I had the subject of the spread of the disease in America under consideration, I was told by one of the physicians there, that the disease rarely originates among their internes or nurses; he stated that he himself had been there for a number of years, and he was quite horrified at the idea that there is great danger in allowing these patients to remain among the members of their families when modern sanitary laws are observed. The results of Dr. Hance's investigations confirm my conclusions that with cleanliness, proper food, abundance of fresh air and sunshine, the danger of infection is very much diminished.

I fully agree with the statement made by Dr. Smith regarding the condition of many of the sleeping cars. Not only the blankets, but the carpets and the draperies are often very much contaminated.

DR. J. W. BRANNAN.—Many interesting and suggestive points have been raised by the paper and discussion this evening, but I shall limit myself to one. So far as I know, Dr. Hance's experiments are the first of their kind in this country, and he has set an example which it would be well for others who conduct sanitariums to follow. These gentlemen invite us to send our patients to their institutions, and it seems no more than just that they should furnish us with a clean bill of health before we do so. I trust they will all receive a copy of Dr. Hance's paper, which is certain to produce excellent results.

DR. A. ROSE.—We have three things in this country

which are missing in those countries in Europe which I have visited; the three things are (1) the carpet; (2) the dark bedroom, and (3) the basement. It would be very interesting if we could calculate how much less tuberculosis we would have if these three things were dispensed with.

THE PRESIDENT, DR. BRYANT.—About five years ago the Chair, who was then health commissioner, addressed a letter to twenty-four medical men in this city, selecting those whom he regarded, by reason of their standing, hospital association, etc., to be best qualified to reply to the question the letter contained, viz.: "In your opinion, is the knowledge of tuberculosis sufficiently far advanced to justify the Board of Health in taking the same cognizance of it as it does of other diseases of a similar nature?"

These twenty-four letters secured five replies. Of these five, two unhesitatingly said "yes," two were doubtful, and one said "no." The trend of the discussion to-night clearly shows that this subject has taken a long step in the right direction since that time, and I trust it will bear great fruits.

DR. HANCE (closing the discussion).—I am wholly in accord with the statement made by Dr. Janeway that patients are often not instructed by their physicians regarding the proper disposition of the sputum. I am anxious to make some further experiments in examining the dust from rooms occupied by consumptives, and if any of the gentlemen present learn of such cases and will send me word, I would greatly appreciate it.

Stated Meeting, December 19th, 1895.

IN CHARGE OF THE SECTION ON OBSTETRICS AND GYNECOLOGY.

VAGINAL VERSUS ABDOMINAL SECTION IN DISEASES OF THE FEMALE PELVIC ORGANS.

By WILLIAM M. POLK, M. D., LL. D.

The rapid evolution of the surgery of the female pelvic organs is one of the great things of this part of our century. Question succeeded question in quick succession until many felt that the end had been reached, and all that remained was the task of perfecting what had been developed. Hardly had we settled down in complacent contemplation of our results with the Trendelenburg posture when we were rudely shaken by this cry of the "vaginal method." Turn from it as often as we may, it yet sings in our ears, and will not be silenced. Intrench ourselves behind a resolution to see in it nothing of good for our patients; view it as a new hobby upon which those of shallow judgment are riding to notoriety, not to honest repute; decry it in all and every way, and it will not down. This is proven by the experience of the past year in this country alone, not to mention what has occurred abroad.

We are to be congratulated that such has been the result of the agitation, for it shows the virility of the subject, and proves that there is a great deal in it for our consideration. As a matter of fact, it is probably the last great question in the surgery of the female pelvis, and deserves to be treated as such by our best men.

Vaginal section has already been injured by exaggerated claims in its behalf. It is folly to talk of driving abdominal section from the field with it, for the reason that conditions will always occur which can be so much better met by the former that no good surgeon would decline to employ it. I think also that the vaginal section will always serve as subordinate to the abdominal, even though it diminish the frequency of the latter one half or two thirds. This belief is based upon the acknowledgment that abdominal section must sometimes be used to complete the work begun through the vagina. In other words, there are cases supposedly entirely amenable to vaginal section, but which demand, as the operation proceeds, a better operative field, which can only be had by combined section. This single admission shows the interdependence of the methods, and suggests that wherever the boundary between them may be drawn, it must be made movable; nothing hard and fast can be tolerated.

It is interesting to recall that vaginal section not so long ago held the vantage ground in this territory, and was driven out except in the case of carcinoma because of its poor results. This was due to its faulty technique in part, and in part to a lack of familiarity with the actual condition which could be present with a diseased uterus and appendages. Through abdominal section it has now informed itself and perfected its technique, and again comes forward for recognition.

Our attitude in this contention can be best expressed by assuming one hundred as the total of all cases of disease in question now recognized as suitable for section. One year ago I thought this could be divided equally between the vaginal and the abdominal. I now think seventy-five can be assigned to vaginal section, leaving twenty-five to be treated by the abdominal.

As the indications for or against depend largely upon the dimension of the diseased area or growth, one must

admit that the earlier the operation is done, the greater the chance for vaginal section. So, assuming that one could observe all cases of uterine and adnexal disease from the outset, it is probable that the ratio would be still further increased in favor of the vaginal route.

Beginning this work at Bellevue hospital in February, 1892, we have now performed seventy-two vaginal sections, with three deaths, covering every species of disorder for which it has been advocated. This ignores vaginal hysterectomies for carcinoma done in the years prior to 1892, for we wish to eliminate from this discussion as much old timber as possible. What we have to say is based upon our own work; work in no way original, however, although it has not been checked by the observations of others, for we tried to approach the subject with an unbiased mind and aimed at independent work, so that when informed from personal experience we could compare conclusions with cotemporaries and with those who had preceded us. It is evident from this statement that we are prepared to modify our conclusions when it can be shown that they are not properly based.

With this understood, we now submit a statement of conditions favorable, on the one hand, to vaginal section, and, on the other, to abdominal section.

EXPLORATORY INCISIONS.—There are certain obscure conditions of the appendages of the uterus and sigmoid flexure in which exploratory abdominal section has become a recognized operation. Accurate vaginal and rectal bimanual palpation under ether is insufficient to discover enough gross lesions to account for symptoms. Direct palpation or inspection is needed for diagnosis. For instance:

CASE I.—A young married woman, the victim of constant pelvic pain and dysmenorrhœa, came to the writer for operation, having been told by competent authority that nothing short of removal of the ovaries would effect a cure.

The pain was referred to the region of the left appendages. Examination without ether revealed what appeared to be a thickening of the tissue at the base of the left broad ligament, the region being very tender. Under ether the thickening was less marked, but there appeared a sufficient contrast with the same region upon the right to leave the diagnosis in doubt, especially in view of the constant, long-continued complaint of pain. A free incision was made into the cul-de-sac. A careful and thorough palpation of all the structures in the pelvis was made with two fingers, and nothing abnormal could be found. The appendages were drawn into the vagina, and inspection, as well as palpation, showed that they were normal. The patient was up in a week, and, as so often happens after an exploratory incision in such cases, was cured by the operation.

CASE II.—Following curettage and trachelorrhaphy an inflammatory mass developed in the outer upper region of the right broad ligament. Supposing it to be salpingitis plus ovaritis, the cul-de-sac was opened with a view to removal or evacuation. To our surprise tube, ovary, and surrounding peritoneal area were normal. But as the swelling was all the more evident it was carefully palpated with a view to operation. The uterus was drawn down by volsella and two fingers were swept over the entire pelvis. First over the posterior surface of the uterus, then the anterior, then along the upper border of the broad ligaments, then down into the paravesical fossæ, thence over the bladder, the anterior pelvic, and lastly the posterior pelvic walls. The following abnormality was in this way mapped out: The outer upper region of the right broad ligament, including its line of connection with the pelvic wall, contained a hard mass; between it and the uterus was a distinct sulcus, in which the tissue, though thickened, appeared in fair condition. To the front, the right paravesical fossa was well nigh obliterated, the outline of the linea terminalis from the attachment of the broad ligament forward to near the symphysis was obscured, all by soft, swollen tissue, which evidently was nothing more than a collection of pus connecting with and springing from the mass in the broad ligament. We were now at liberty to select an exit for the pus. This could be had in

two ways. A direct incision above Poupart's ligament, turning up the peritoneum, reaching the pus about the pectineal eminence. Another route, and the one selected, was directly from the vagina. The cul-de-sac was closed; then, in order to reach the pus and at the same time avoid the ureter, an opening was made as in anterior colpotomy; this was extended beneath the peritoneum, upward and outward to the region of induration, whence the pus thus freely escaped.

DISPLACEMENTS FIXED BY ADHESIONS.—From several cases we select one:

CASE III.—The uterus was retroverted, ovaries prolapsed, and both, with the tubes, were bound down by firm and old adhesions. For two years prior to operation the patient had been subjected to vaginal tamponade for the purpose, as was said, of stretching the adhesions, so as to permit the uterus and appendages to be raised by a pessary. The folly of this policy was never better illustrated, for the organs remained, as they always do, in the abnormal position. Free incision of the cul-de-sac gave easy access to the structures. These were readily stripped of their adhesions, and then by means of Alexander's operation they were permanently placed in proper position. Easy and speedy recovery marked this case.

OVARIAN TUMORS.—All such tumors small enough to be contained, wholly or in large part, in the pelvic cavity should be removed by vaginal section. Tumors large enough to reach beyond the umbilicus, especially if they are pedunculated and are wholly outside the true pelvis, can be best treated by abdominal section. This observation applies with greatest force to multilocular colloid growths, but even then it is susceptible of modification in favor of vaginal section if it can be shown that both ovaries are hopelessly diseased. Then hysterectomy being permissible, sufficient opening is secured to evacuate properly and withdraw even such colloid growths. If this be true of these latter tumors, it applies with greater force to unilocular cysts with more fluid contents. From this it must

appear that while tumors wholly outside the pelvis can be best treated by abdominal section, many of these can be reached by the vagina, provided it be proper to remove the uterus. In these cases it is wise to operate with the hips somewhat raised, else the intestines and omentum will occupy the field, and are apt to cut off the escape of fluid, which then tends to ascend and accumulate just under the abdominal walls.

CASE IV.—*An intraligamentous ovarian cyst* of the left side, with dimensions about equal to the fœtal head at seven months, was removed per vaginam through the cul-de-sac. Some difficulty was experienced in enucleating the sac. This was overcome, however, by opening the anterior fornix, through which the sac was easily removed. Our experience in this case impressed us with the advantages of the anterior over the posterior line of approach in the intraligamentous growths. By this route most of them can be removed without entering the peritoneal cavity. After reaching the under surface of the peritoneum in the utero-vesical fold, it is only necessary to push aside the tissue with the finger, when the wall of the sac contiguous to the lateral uterine wall is easily reached and punctured. Subsequent enucleation can then be readily made. By hugging the uterus well up to the region of the body before attempting puncture of the sac, one easily avoids ureter and bladder. Hemorrhage is readily controlled by forceps.

CASE V.—*A small pedunculated ovarian cyst (size of fœtal head at term).*—The cyst was adherent to the pelvic floor, was therefore readily reached and quickly removed. All such cases are eminently fitted for the infrapubic operation. Solid or fluid, hard or soft, benign or malignant, the route through the cul-de-sac gives easy access and ready control of each and all.

CASE VI.—*Extra-uterine pregnancy.*—This was an example of an extra-uterine fœtation, in which incision, evacuation, and drainage brought a speedy cure. Bleeding points were many, but they were secured by forceps, which were left in place forty-eight hours. This is a condition which, prior to rupture, one can always elect to

reach by the vagina. The tumor from first to last is well down in the posterior regions of the pelvis, and is easily reached through the posterior vaginal wall. It may be necessary to remove the uterus, however, for without this addition we may have excessive hemorrhage. Still, if one can get hold of the connection of the mass by clamping upon its two sides, outer and inner, bleeding can be stopped. After the placenta is fully formed, the child being alive, the suprapubic route would no doubt be preferable. Touching those cases, whether early or late, in which from antecedent rupture or otherwise the fœtus is dead, the infrapubic route will meet every requirement, as we have then little more than to evacuate and drain.

INFLAMMATION AND SUPPURATIVE DISEASE OF THE APPENDAGES, INCLUDING TUBERCULAR DISEASE.—This field is particularly rich in opportunities for vaginal section. In fact, there appears to be no stage which positively contraindicates it. It offers the best means of checking the ravages of acute inflammation, thus tending to the highest kind of conservatism; it affords opportunity for the partial plastic operations upon the adnexæ and uterus, and it gives us the best operation for suppurative disease of these same adnexæ, when their removal is demanded, as in tubercular disease and in the destructive inflammation of both appendages.

It is interesting to note that this class of cases has furnished the battle-ground of this question. But the increasing belief that the uterus must go with the appendages, that hysterectomy in destructive double adnexal disease is required, and further, that plastic so-called conservative operation upon the uterus and appendages can be done through the vagina, bids fair soon to settle the question in favor of vaginal section.

CASE VII. *Acute processes.*—This is an instance of acute salpingitis and pelvic peritonitis following abortion at two and one-half months. The uterus was cleansed and packed with sterilized gauze; the cul-de-sac by a free

opening evacuated of turbid serum. The inflamed appendages were not disturbed, a loose gauze drain being carried into the cul-de-sac, which was first washed out with sterilized water. Finally, the vagina was loosely packed with gauze. This was removed at the end of forty-eight hours, the final result being all that could be asked.

This case illustrates what will probably be a common application of one step of the infrapubic operation, namely, the incision into the cul-de-sac with a view to drainage. This step appears to promise much in mitigating the damage which befalls the appendages in the face of the inflammations which come to them through those very common causes—abortions and gonorrhœa. The cleansing of the uterus, together with free drainage from the pelvic peritoneal area, seems to be the rational way of treating such cases, but it is a self-evident proposition that to be of service it must be done early.

CASE VIII.—*Acute puerperal metritis, etc.*—In connection with the preceding case the present one illustrates the ease with which one may go to extremes when necessary in puerperal septic cases. A woman was septic five days, the form being sapræmia rather than mere sepsis. In ten minutes the uterus was removed and the operation completed. The relaxed and dilated state of the whole genital tract makes the operation exceptionally easy. From this we infer that all such cases are peculiarly fitted for the infrapubic operation.

CHRONIC PROCESSES.—For illustration, a synopsis of three cases is now given.

CASE I.—By means of *anterior colpotomy* the uterus was anteverted, the fundus and the appendages were brought through into the vagina. The right appendage was normal, the left diseased. The left was removed. The remaining organs were then returned to the peritoneal cavity. The opening in the peritoneum was closed, that in the vaginal wall left open. The uterus was curretted and packed; patient out of bed in a week.

CASE II.—By means of *posterior colpotomy* an adherent and purulent ovary and tube were discovered on the left side. They were removed, a clamp being used. Upon the right side the appendages, being normal, were not removed. The uterus was treated as in Case I, and the patient did quite as well.

CASE III represented a double ovarian abscess and double pyosalpinx. The pelvis was practically filled with the mass and the associated adhesions. The uterus was first cut out, and then the left and, lastly, the right appendages were removed. The process was tedious, and, owing to the intimate and firm attachment of the rectum, this intestine was torn. No attempt at closure was made, as the opening was small. The recovery was quick, but a fecal fistula remained for two months, when it finally closed spontaneously.

FIBROID DISEASE OF THE UTERUS.—The tumors when of small size are more amenable to vaginal section than almost any other condition. This has already been well brought out at the Obstetrical society in this city, to whose proceedings I refer for more specific remarks in this direction.

In brief, it may be said that all fibroid tumors lying chiefly in the pelvis are suitable for morcellation by the vagina, the intraligmentous for instance. Pure myomas and fibro-cystic tumors, even though they extend as high as the umbilicus, may also be brought within the limits of vaginal section, but not so with the hard, pure fibroid growths. It is difficult to fix any limit for these, but in general it may be accepted that where such tumors are wholly above the pelvis and fill the hypogastric region they had better be removed by abdominal section. Especially is this the case if the pelvis is narrow and deep; but we find much encouragement to the removal of these growths by the vagina when we contemplate the remarkable work accomplished for so many years in the removal of submucous fibroids by vaginal morcellation.

OBJECTIONS.—After all has been said it is plain, however, that there are objections to vaginal section which are sufficient to deter many operators from adopting it. Let us see what they are. The conformation of the pelvis does exercise a decided and, it may be, a controlling influence. All deep, narrow pelves render vaginal section difficult, and in the case of partial operations, such as removal of but one appendage, when thickness and rigidity of the pelvic floor, as may be met with in some women, is added, this will practically forbid it. In the presence of a male pelvis in a stout woman with a narrow vagina one should be slow to adopt vaginal in preference to abdominal section. In this connection I quote from an article already published in the *American Journal of Obstetrics*, Vol. xxxi, page 5:

The anatomical condition which presented to me the greatest difficulty was that found in large women with the male pelvis. The deep pelvis with its comparatively narrow outlet was usually found to contain a uterus with unusually thick and inelastic utero-sacral ligaments. I also found in some of these cases a considerable depth of tissue (fat and connective tissue) in the make-up of the pelvic floor. Douglas's cul-de-sac was deep and narrow, and the lateral fossæ of Douglas were wide and roomy. It is in these lateral fossæ that the diseased appendages commonly rest; under the above conditions it was not always easy to reach and enucleate them and then draw them into the vaginal opening. Such a pelvis, however, would offer no special obstacle if the condition demanded hysterectomy, for then the prior removal of the uterus would give easy access to the appendages, no matter where located within the true pelvis. After such removal the opening at command would be almost equal to the confines of the recent pelvic excavation, and through it, as has been shown here, large pus-tubes and ovarian abscesses could be safely removed. Clearly, then, an element of obstacle in this work is the uterus itself. If the operator planned to remove this organ as a part of his operation, he would have before him no greater procedure than a vaginal hysterectomy, one, too, which was far simpler than the same procedure when executed for carcinoma. A vaginal hyster-

ectomy for cancer generally meant as much encroachment upon tissues and organs about the uterus as was compatible not so much with the integrity of adjacent structure as with the life of the patient. A vaginal hysterectomy for inflammatory disease of the uterus and appendages and for fibroid disease meant, on the contrary, freedom to hug the diseased tissue as closely as possible. In the one, bladder, ureters, and rectum might be in constant danger; in the other, they are excluded from the field with the utmost ease. No adventitious guides or guards are needed; such things become, in fact, a hindrance rather than an aid.

Referring again to partial operations upon the female pelvic organs, such as salpingotomy, oöphorectomy, resection of ovaries, excision of pyosalpinx, etc., I find that the thinner the subject and the more shallow and roomy the pelvis, the greater the ease of procedure. Another factor likewise exists, as already mentioned, in the make-up of the pelvic floor. The thinner this is, the easier becomes manipulation.

The claim is made that the operation is liable to be incomplete in the case of pus-sacs. No doubt this is true of the past, and perhaps at the present with some operators, but the youth of the operation accounts for this. The same objections formerly held good with abdominal section in such cases, as is shown by the number of inoperable cases that used to be reported.

Another objection is that viscera are more liable to be injured. This relates chiefly to the rectum, and is no doubt true, but in spite of it the cases do as well ultimately as similar ones done by abdominal section. Such cases when treated by abdominal section require drainage, and prolonged drainage, as a rule. This predisposes to hernia, so that against a temporary vagino-intestinal fistula in vaginal section one must put hernia with abdominal section.

Herniæ are not common after vaginal section, for if they were we would have heard more of them, in view of the long time vaginal hysterectomy for cancer has been a

recognized and oft-performed operation. The claim that sepsis is more common is not borne out by the experience of operators. It is true that the average temperature after vaginal section is higher for the first two or three days than after abdominal section, but this applies only to cases in which the forceps instead of ligatures are used.

The same comment applies to the objection urged on the score of the offensive vaginal discharge. Here again the claim rests against the forceps rather than the ligature. But, after all, the objection is one readily controlled by proper vaginal douching, and is relatively an unimportant objection, when the advantages of vaginal section are considered.

ADVANTAGES.—These briefly stated are: As much safety as abdominal section, and this, too, at its inception, leading one to believe that with the same relative time and attention given to it that abdominal section has secured, it will be safer. The recovery is more rapid and the after-condition is better, because herniæ and omental and intestinal adhesions are less common.

Finally, we beg leave to say that, from all that has been said and done in this subject by the various operators, we might as well admit that a revolution is taking place in our methods of dealing with the uterus and appendages, and it is safe to say that most of the operations done now above the pelvis for conditions specified in this article will soon be done from below. It is merely a question of learning how to do it.

OPERATION.—The diet should be light the day before, food being taken as late as six hours prior to operation. Twenty-four hours before the operation a brisk cathartic should be given. Six hours before operation the lower bowel must be thoroughly washed out by a copious enema of castile soap and warm water. The bladder should be carefully emptied at time of operation. The pudenda should be shaved; the vagina and cervix should then be

cleansed, green soap and water being thoroughly applied with a brush. This measure must never be omitted, but if one case calls for the brush more than another it is the vir-

FIG. 1.

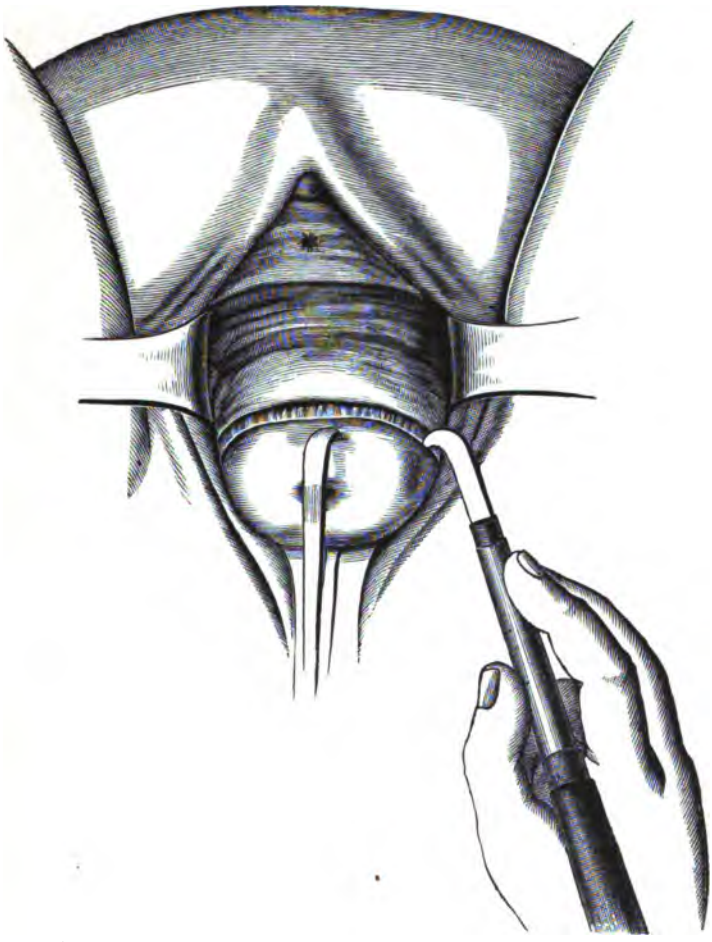


Fig. 1. Vaginal hysterectomy. Initial application of cautery knife.

gin vagina, the vagina with rugæ. Of course, the upper surface of the external genitals, the vulva, the anal regions, and the buttocks must be subjected to the same process of

cleansing, the brush being used sparingly on the delicate structures of the clitoris and vestibule. The entire surface, internal and external, must now be washed off with 1 to

FIG. 2.

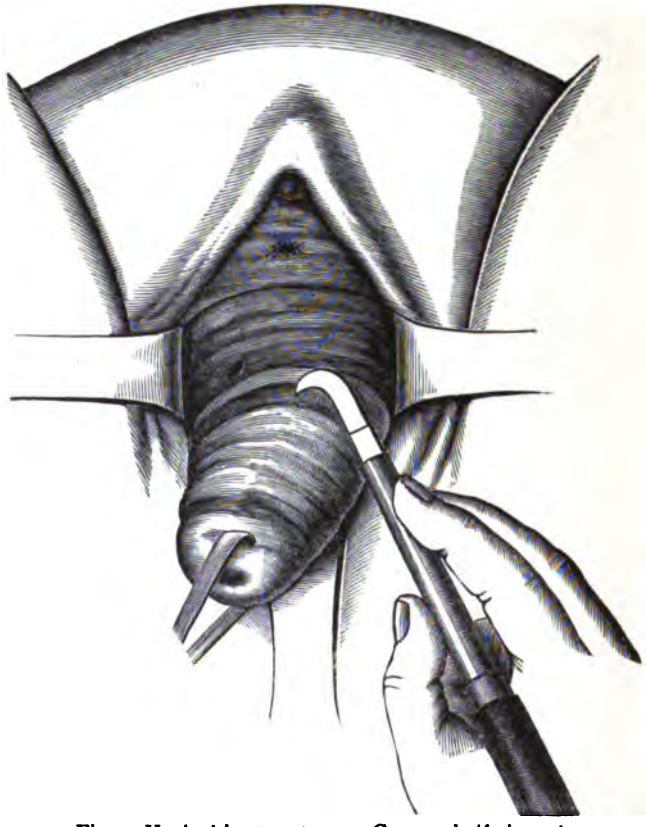


Fig. 2. Vaginal hysterectomy. Cautery knife in action.

1,000 bi-chloride of mercury solution, and this in turn washed away with warm sterilized water.

When all this has been carefully executed, the patient is ready for any one of the several operations done by the vagina.

If the uterus is to be retained, and is to be curetted and

packed, this step is now carried out. The debris of the curettage is then removed by washings of 1 to 2,000 bichloride of mercury, and the patient is ready for the next step. Let us suppose this to be an operation upon the ap-

FIG. 3.

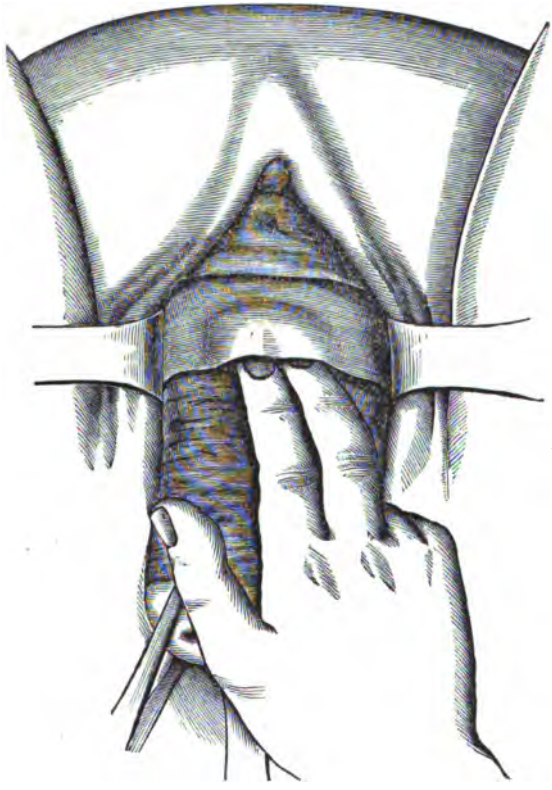


Fig. 3. Vaginal hysterectomy. Enucleating with fingers after preliminary cutting of vaginal wall with cautery knife.

pendages—some plastic or conservative procedure. In that event the operation is anterior colpotomy.

The body of the cervix is caught with a volsella just below the anterior utero-vaginal junction. Strong traction is then made (Figs. 1 and 2), while with the cautery knife a

transverse incision is carried through the vaginal wall from one side to the other about coincident with the above junction. If the uterus is small, this incision need go no further than the sides of the cervix, and by this we mean the line of junction of the anterior and posterior halves of the cervix; but if it is large, it should be extended thence downward and outward half an inch or more, onto and through the latero-posterior vaginal wall. The bladder is next separated from the uterus, using the index and middle finger for this purpose (Fig. 3) and the utero-vesical fossa is entered. Sweeping the fingers right and left, the bladder and ureters are pressed well aside. Still using the two fingers, the several viscera, including uterus, broad ligament and appendages, intestines and bladder are carefully palpated. Finding the conditions favorable to plastic or conservative operations upon the uterus or appendages, we proceed according to the problem presented.

If it be the enucleation of a sub-peritoneal fibroid on the anterior face of the uterus—that is, anterior to the broad ligaments—we proceed to this at once, holding the field of operation against the vaginal incision by pressure from above. If it be the removal, in whole or in part, of an ovary, a tube, or the enucleation of a fibroid at the fundus or on the posterior face of the uterus, posterior to the broad ligaments, we antevert the uterus and bring the fundus and body through the vaginal incision into the canal (Fig. 4). This can be best done by bringing through first one of the cornua, after which the remainder of the organ is made to follow. Several instruments have been devised to further this step, which is the most difficult in the operation, but nothing is so good as the two fingers of the operator working through the incision, aided, if necessary, by those of an assistant, placed in the rectum for the purpose of keeping the uterus forward. After the organ is drawn well into the vagina we act in accordance with the problem before us. Fibroids are enucleated upon the same

plan as prevails under other circumstances—that is, the capsule is cut through and the growth seized with a volsella, and is worked out of its bed with the aid of the fingers and the scissors. Deep sutures of catgut or fine silk are then passed, for the double purpose of closing the opening left and checking hemorrhage. If the ovary is

FIG. 4.

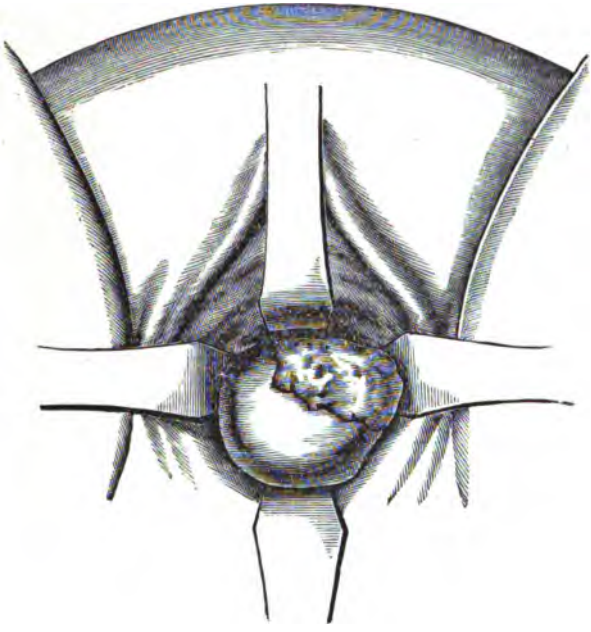


Fig. 4. Anterior colpotomy. Fundus uteri and left ovary drawn into vagina.

to be operated upon or removed, it is drawn through into the vagina (Fig. 4,), being reduced in size where necessary by aspiration or incision. When in the vagina it is at the command of the operator, and can be removed in whole or part as need demands. These statements are applicable to the tubes, which should also be brought out into the vagina, being previously emptied of fluid contents by aspiration.

The ligation and removal of an ovary (which should be done independently) are, in general, simpler than the same operation upon the tube, because of the more extended blood-supply of the latter. In removing a tube,

FIG. 5.

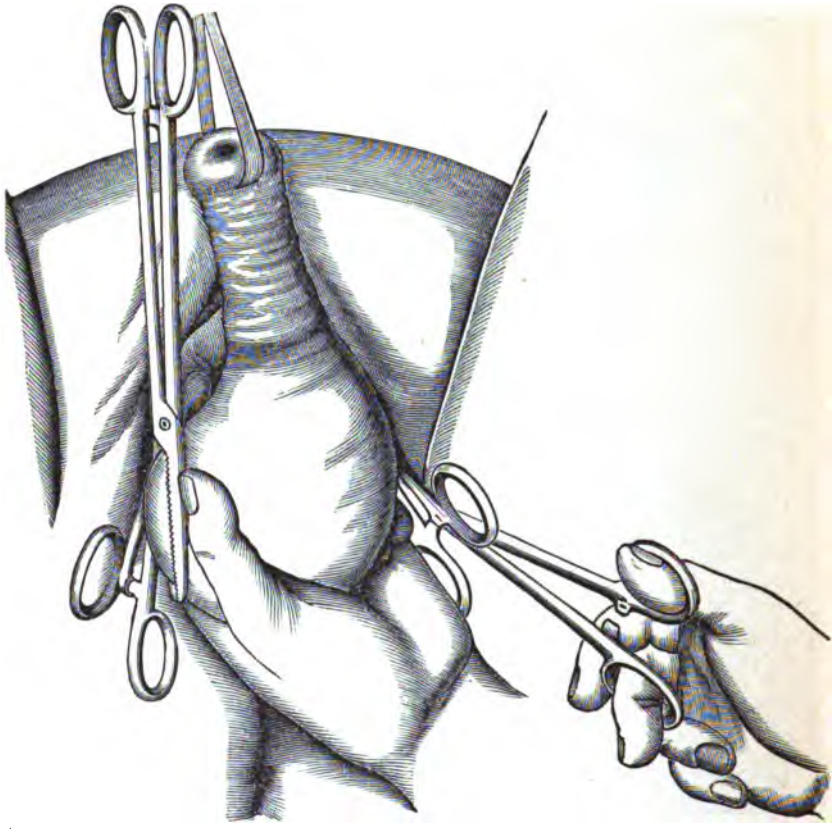


Fig. 5. Vaginal hysterectomy. Forceps applied to lower segment of the right broad ligament and ovarian artery.

tie as much of the mesosalpinx as you can, this generally is all of it; then ligate the uterine end of the distribution of the ovarian artery by including every structure at the cornu, except the tube; hug this closely with your liga-

ture. Next enucleate the tube, and tie such bleeding points as appear. Having completed the operation, the uterus is returned to the abdominal cavity, and the divided peritoneum of the utero-vesical fold is reunited with catgut suture. If the operation has not involved bruising of the vesico-vaginal connective tissue, the cut in the vaginal wall may also be closed. If the contrary has been the case, it is best to leave the cut open. It may be sometimes necessary to pass from anterior colpotomy to hysterectomy, as where, for instance, we discover that both tubes are purulent and that the ovaries are similarly involved. This can be easily done by carrying the incision around the entire utero-vaginal junction, and then proceeding to the removal of the uterus, as we shall next describe under vaginal hysterectomy, with evacuation and removal of diseased appendages, such as pus-tubes, ovarian tumors, etc.

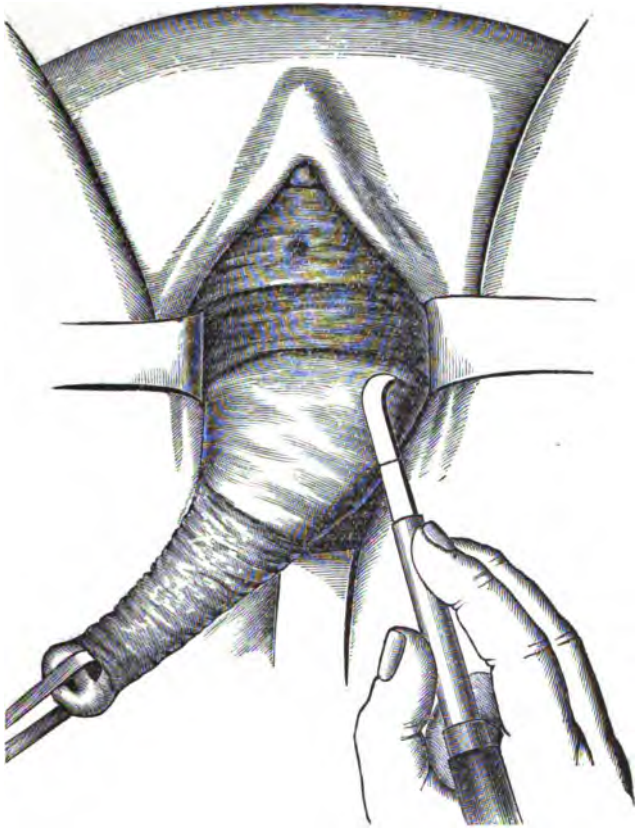
POSTERIOR COLPOTOMY.—This section is resorted to for purposes of simple drainage, as in acute affections involving the cul-de-sac. It is also a part of the operation which looks only to the evacuation of pus-sacs, the removal of small ovarian tumors, the severing of adhesions, and exploration of the pelvis. It consists of a vertical section of the posterior vaginal wall, extending from the utero-vaginal junction to the bottom of the cul-de-sac. It can be made with knife or scissors, as there is small risk of hemorrhage, and, as a rule, can be closed when the work is done, unless pus or some such infecting substance as the contents of a dermoid has been encountered.

VAGINAL HYSTERECTOMY, WITH EVACUATION AND REMOVAL OF DISEASED APPENDAGES, SUCH AS PUS-TUBES, OVARIAN TUMORS, ETC.—It is assumed that the patient has had the usual preparation, that the bladder and rectum have been emptied. A soft rectal bougie should now be carried into the rectum and passed as far as it can be gotten to remain. The vulva and the vagina having

been cleansed, as already described, and the cavity of the uterus, and especially the cervical portion of it, having been curetted and washed out with a solution, 1 : 1000, of bichloride of mercury, the removal of the uterus should be the next step. The cervix is grasped back and front with a blunt volsella and drawn forcibly down as near the vulva as possible. The vagina at the utero-vaginal junction is then cut through entirely around the uterus (Fig. 1). This accomplished, the enucleation of the cervical portion is next in order (Fig. 2), to be followed by measures designed to control hemorrhage. If it be by ligature, this is passed first on one side, then upon the other, so as to secure the uterine artery, which can be easily palpated. If, on the contrary, the clamps be preferred, one is placed on each side, so as to secure the entire lower half of the attachment of the broad ligament to the uterus, which will include the artery. These clamps are made to hug the uterus as closely as possible, and are guided into position by the thumb and index, or index and middle, fingers, thrust one in front and the other behind the broad ligament, at the uterine attachment. The vessels of the lower segment of the uterus being secured by clamp or ligature, the organ is cut free and the enucleation is carried on with the index and middle fingers (Fig. 3) until the bladder is separated and the utero-vesical space in front and the cul-de-sac behind have been opened into; the vessels at the sides are not molested, the vaginal wall being merely pressed back to give easy access to them when ligatures or clamps are to be applied. If the case is simple, that is, if there is no extensive matting together of the visceræ, as in bad cases of suppurative disease, the peritoneum upon the base of the bladder and in the cul-de-sac should be attached with a single stitch (catgut) to the corresponding vaginal wall. The way is then clear for the application of the clamp or ligature, as one may elect, to about its middle, still applying steady traction; the upper attach-

ment of the uterus is next brought down; this is clamped (Fig. 5) or ligated, after the manner already described, and then the entire uterus is cut out. The succeeding steps depend upon the condition presented. If it be a

FIG. 6.



Vaginal hysterectomy. Enucleation of uterus with cautery knife without clamp or ligature.

simple pyo- or hydrosalpinx, these, along with the degenerated ovaries, are enucleated by the fingers, aided by long forceps, and are then clamped or ligated at the attachment to the broad ligament and cut away. If it be

an ovarian tumor, this is drawn down to the cut in the vaginal roof, is there tapped, and after the contents have been removed the sac is withdrawn much after the manner which prevails with abdominal section.

The serious difficulties arise in connection with the conditions of pelvic abscess. We may not be able to remove the uterus as cleanly as has been described, but may be forced to take it out by morcellation. Cut it out segment by segment, clamping the bleeding vessels as they appear. The posterior lower half is first removed, then the anterior. In this procedure we may open promptly into a pus-sac in the cul-de-sac. After cleansing this out, we proceed to the upper half of the uterus. This should be cut open antero-posteriorly. After removing the two segments, we use the cut ends of the cornua as guides to the pus-sacs, tubes, and ovaries of either side. Feeling one's way carefully, making traction upon the sacs, which by this time can be differentiated, first one side then the other is drawn down, enucleated, and withdrawn. It may be that this process of enucleation will have to be done in the depths of the pelvis; if so, one cannot use too much caution, for it is in such cases that visceral injuries are most liable to occur. Patience and intelligent perseverance will enable one to surmount the difficulties, however, so it is rare that one will be forced to call to his aid abdominal section. This, however, should be done if the intestines high up in the pelvis have been injured. Having opened above the pubis, the patient should be thrown into the Trendelenburg posture and the injured intestine quickly found and repaired. If any sacs, or parts of sacs, remain they should be taken out; the pelvis must then be washed out with sterilized water and a loose gauze drain placed in the bottom of the pelvis, extending thence through the vagina.

After vaginal hysterectomy of any kind it is wise to leave the vaginal cut open, draining through it by means

of a good strip of gauze, which extends from the depths of the cul-de-sac to a proper dressing over the vulva. This precaution may be optional in clean cases, but it is an absolute necessity in all pus-cases.

If ligatures have been used, they are left long, are tied together, and placed in the vagina. If clamps are employed, they are looked over carefully to see that each is secure, doubtful ones being further guarded by tying the handles firmly together. The handles of the clamps are then encircled with a thick layer of absorbent cotton, which is also in contact with the vaginal drain, whose function it furthers. A soft-rubber retention-catheter is finally placed in the bladder, and the operation may be said to be complete. The catheter is self-retaining, by reason of a bulb or collar at the visceral end of the instrument, but to realize its object it must be plugged or knotted at its free end. By removing this plug or knot at intervals of two or three hours the bladder is properly emptied and the dressings are kept clean. This upper cotton dressing and the catheter are the same for all cases of vaginal section.

This article would not be complete were we to omit mention of the method of removing fibroids by morcellation.

The preliminaries up to and including separating the vagina from the cervix are identical with the process already described. The departure begins after the cervix has been enucleated, and the lower vessels ligated or clamped. This consists in cutting away, piece by piece, first the cervix, then the body of the uterus, dragging each piece forcibly downward with blunt-toothed volsella; bleeding points are caught as they appear and ligated or left in the keeping of the clamps, as convenience dictates. Working after this fashion, with knife and scissors, protecting the vaginal walls with retractors, the tumor is dragged piece by piece into the vagina, and then cut off. In this way the operation is really performed within the

vagina, sparing the cavity above it. As already intimated, the dressing and later treatment of the case coincide with those adopted in other vaginal sections.

A word must now be said about enucleation without clamps or ligature. It can be done, but it has its limitations, and should be performed with the cautery. We confess that a ligated or clamped vessel makes us less anxious, and as there is no valid objection which is at all weighty, we prefer to use one or the other.

The best cases for this operation are women past the menopause or who have already been rid of their appendages—the uterine vessels being with all such persons more or less atrophied.

It is done as follows (Fig. 6): After the usual preparation, seize the uterus at the cervix and burn through the vagina at the utero-vaginal junction. Now keep up forcible and continuous traction upon the cervix, at the same time burning through the attachments to the uterus as they descend. This burning must be made within the true uterine tissue rather, the cautery being driven firmly into it. It is best after each segment has been burned through to relax the traction, to see if the vessels just severed bleed; if they do, sear them at once, and continue it until they are closed. Proceeding in this manner the uterus is quickly peeled from its envelope, and the cornua are brought into view. These are burned through and their stumps seared. In the removal of the tube and ovary it is wiser to use ligatures or clamps. This having been done, the operation is complete. The after-dressing and treatment are the same.

Speaking now of vaginal hysterectomy in general, clamps should be removed in from forty-eight to seventy-two hours, after which the patient should receive a night and morning douche of warm boric acid or permanganate of potash solution, for purposes of cleanliness and comfort. Ten days cover the usual limit of convalescence in bed,

the patient being permitted, under ordinary circumstances, to take the sitting position in bed at the end of a week. The catheter should be removed when the clamps are taken off. Ligatures should be cut out at the end of two or three weeks. The bowels are treated much as in abdominal section, but there is one comfort the patients with vaginal section can enjoy, that is, morphine for the relief of pain. This drug does not appear to have as troublesome consequences after this form of operation as after abdominal section. With this, Mr. President, I must close.

Recapitulation of conditions favorable on the one hand to vaginal section, on the other to abdominal section in diseases of the female pelvic organs:

VAGINAL SECTION.

1. A shallow and wide pelvis in a thin woman.
2. Explorations of the pelvis.
3. Visceral adhesions in true pelvis.
4. Displaced and adherent uterus.
5. Smaller ovarian cysts, especially the intraligamentous and parovarian.
6. Smaller fibroids, especially the soft.
7. Extra-uterine pregnancy, up to seventh month, and after death of fœtus.
8. Pelvic hæmatocele.
9. Puerperal hysterectomy.
10. Acute inflammation of appendages, with peritonitis involving cul-de-sac.
11. Inflammatory destructive diseases of the appendages, including tubercular disease.
12. Pelvic abscess pointing downward.
13. Conservative operations on appendages that lie in true pelvis.

ABDOMINAL SECTION.

1. A narrow and deep pelvis, especially if deformed.
2. Explorations above the true pelvis.

3. Visceral adhesions in false pelvis or above.
4. Large ovarian cysts, especially multilocular, with colloid contents.
5. Large fibroids, especially the firm and hard.
6. Extra-uterine pregnancy at time of rupture and at term.
7. Extra-uterine pregnancy, with tumor wholly above brim of pelvis, and not in relation with uterus.
8. Pelvic abscess pointing upward.
9. Conservative operations under conditions unfavorable to vaginal section, such as a narrow and deep, or a deformed pelvis, that is contracted.

DISCUSSION.

DR. E. W. CUSHING, of Boston.—There can no longer be any doubt in the minds of those who have studied the recent developments of the technique of vaginal hysterectomy, that the resources of our art have been wonderfully increased by the introduction of the method of morcellation of the uterus.

It is a significant fact that the use of this procedure for salpingitis, pelvic suppuration, and myoma has extended in the face of strong opposition, and that it has increased from year to year in Europe, until now its most ardent advocates are those who formerly declared that it was far inferior to abdominal section, as a method of operating for these disorders.

In this country there are as yet few advocates of this extended use of vaginal hysterectomy by morcellation; but its opponents are mostly those who have neither seen the perfected operation, nor comprehended in what way it is better than vaginal hysterectomy as hitherto performed. There are many who agree with an eminent surgeon in this country who has not hesitated to declare that vaginal hysterectomy for pus tubes is "blind, ignorant, and cowardly."

I must confess that before I had investigated the matter I was somewhat of the same opinion, and, in reading in the French journals the accounts of the use of this operation for pyo-salpinx, I felt that something like a crime against good surgery was being committed.

As time passed, however, it was evident, both from the character and ability of the men who were advocating and

using this method, and from the results published, that a new era had commenced in pelvic surgery, and that great changes in our methods were impending. Therefore, last summer, I availed myself of the courtesy of Professors Péan, Ségond, Bouilly, Richelot, and Aubeau, who gave me every opportunity to study their methods and results, and I was much impressed with the merits and advantages of vaginal hysterectomy by morcellation both for myomata and for salpingitis and pelvic suppuration.

In discussing with Professor Ségond the relative merits of the methods of performing hysterectomy most used in America and in France respectively, we agreed that while in this country during the last five years, the technique of abdominal hysterectomy had been brought to great perfection, yet the concentration of surgical effort on this subject had led to comparative neglect of recent improvements in the vaginal method by morcellation, whereas in France exactly the reverse had taken place. Consequently, since every operator places most confidence in the method with which he is most familiar, and in which he has developed most skill, there is no doubt that in each country cases are operated on by the method in vogue there, which really could be better treated by the other procedure. Therefore it behooves the surgeons of each country to study and practise the method in use in the other, and then the conditions can be determined which would properly indicate whether in a given case abdominal section or vaginal hysterectomy should be employed.

The claims and arguments of the advocates of vaginal and abdominal section for pyo-salpinx, respectively, have been repeatedly urged during the last years at the meetings of the Surgical Society of Paris, the French Congress of Surgery, and the International Congress of Gynæcology, and elsewhere.

The first represented by Ségond contends that "Utero-ovarian castration should be preferred to ovarian castration by laparotomy for three principal reasons: 1st. The operation is less severe; 2d, it gives more complete cures; 3d, it avoids all inconveniences of a supra-pubic cicatrix.[1]

To these claims of Ségond, the advocates of laparotomy reply by arguments which have been tersely summed up

[1] Ségond. Bull. and Mem. de la Soc. de Chir., 1891–XVII–153.

by Baudron as follows: 1st. Laparotomy is not more severe than vaginal hysterectomy; 2d, it gives perfect results which time has shown to be permanent; 3d, it is more complete than hysterectomy, for if it leaves a useless uterus, vaginal hysterectomy deliberately abandons those pockets which cannot be enucleated by that method; 4th, it does not expose one to the risk of wounding such important organs as the bladder, the ureter, the rectum, the small intestine, etc.; 5th, it is an operation which can be controlled, which permits one to stop at any time and to preserve the fecundity of the woman, if this be possible; 6th, finally the scar is no longer a deformity, thanks to the perfection of the suture, which makes it not even necessary to wear a belt.

It would carry me too far in this brief paper to enter on a discussion of each of these arguments, especially since the statistics already published show that accidents to other organs are not so frequent with the vaginal as with the abdominal method. I will merely mention that the march of surgical progress has been so rapid in this country that many of our most distinguished surgeons are no longer satisfied with simple removal of the uterine appendages for pyo-salpinx, but prefer to perform total extirpation of the uterus and appendages by abdominal section, in order to be free from the evils which are caused in so many cases by leaving a heavy suppurating uterus, and all admit that it would be desirable to remove the uterus in such cases if it were not for the increased shock and danger thereby incurred.

The question is therefore properly between total abdominal versus total vaginal extirpation of the uterus and appendages.

I shall content myself to-night with stating the reasons which led me, after witnessing a large number of vaginal hysterectomies, to conclude that the claims of the advocates of this operation were well founded, and accordingly have caused me to perform the operation a good many times after my return from Paris, with results which have confirmed favorably my opinion of this method.

1. In the first place all conclusions or arguments based on the performance of vaginal hysterectomy as it is, or until recently was, ordinarily done in this country are of no value, for by morcellation the character of the operation is

so changed and its facility and scope so much increased that it becomes in effect a new procedure. The novelty is based on the fact that by splitting the uterus, either half can be brought down separately much farther than the organ as a whole can be made to descend.

2. By removal of the posterior segment of the uterus, after splitting it laterally, room is gained in the lower pelvis, which permits the remainder of the organ to be tilted or rotated forward, thus exposing more of the anterior surface than was otherwise visible. By splitting this anterior part in the medium line each half is made to descend again further, and when what is thus accessible is removed it is possible to begin the same process of section, anterior rotation, and ablation on the remaining portion of the uterus, until so much is removed that the remainder of the organ is easily delivered.

3. During this gradual descent and anterior rotation of the organ any adhesions between the uterus and the intestines or appendages come into plain sight in the anterior cul-de-sac, and can be separated with the utmost care, leaving, if necessary, the peritoneal surface of the uterus adhering to the bowel but respecting the integrity of the latter.

4. In this method the vessels of the broad ligament are controlled by clamps with short and powerful jaws applied under full control of sight and touch, without danger of injuring the intestine or ureters; the latter with the bladder are held out of harm's way by a thin retractor hooked under the arch of the pubis.

In this way, therefore, the uterus can be safely and speedily removed with a minimum of shock, even in cases where it is firmly bound by adhesions, or where it contains a myoma. In the latter case, as soon as the cervix is wholly removed the capsule of the tumor is opened, and the myoma presents itself. This is then pulled down, incised and cut away, piece by piece, working always within the capsule, so that the abdominal cavity is not opened, and the intestines are not seen until the capsule or uterus, from which the myoma has been extracted, is delivered. By this means all fibroid tumors up to the size of a cocoanut can be removed with far less shock than by abdominal section.

If it be granted, however, that by morcellation the uterus

can be removed more quickly and safely through the vagina than by abdominal section, it remains to be proved that it is feasible and judicious to remove the diseased uterine appendages from below rather than from above. The ease and safety with which this can be done, however, the facility and delicacy with which adhesions to the bowels can be separated in plain sight, can hardly be believed, at least by one whose experience has been with abdominal section only, until he sees the vaginal operation performed by the hand of a master.

As soon as the procedure is witnessed, however, it is seen that the anatomical connections of the uterine appendages particularly favor the vaginal method of removing them, room having been gained by ablation of the uterus.

Whatever be the pathological adhesions of the tubes and ovaries, their attachment to the pelvic wall is always so low down that when liberated from adhesions, they may readily be brought outside the vulva. Whatever part of the tube presents itself, therefore, after removal of the uterus, may be gently drawn down and enucleated from all adhesions by the finger, not blindly, but by sight. Usually the diseased tubes have been situated down behind the uterus where they are readily accessible after removal of the latter. If full of pus they may be opened and evacuated and washed out, without soiling the intestines, which have been protected by sponges. Then by gentle traction the sac is drawn down, if necessary removing one portion after another; the bowel follows if adherent, and the adhesions are separated under the guidance of the eye.

To be sure, in very grave cases the sac of the tubo-ovarian abscess may have to be left *in situ*, but in this case it can be cleaned out, even curetted, washed out, and drained with gauze much better than can be done through an abdominal incision. In some cases the general abdominal cavity is not even opened at all.

In fact, one of the greatest advantages of vaginal hysterectomy is the perfect drainage which can be secured, and this is probably one of the reasons for the excellent results obtained by this method, which compare very favorably with those following abdominal section.

It remains to consider what circumstances would indicate the choice of either the vaginal or the abdominal operation in any given case, supposing the operator to be

equally familiar with both methods, and provided with instruments suitable for performing either.

First, as regards myoma of the uterus, I think there is no question that the vaginal operation is to be preferred when the tumor is of moderate size, *i. e.*, up to five inches in diameter, more especially if it be incarcerated in the pelvis, or infra-peritoneal in its developments. The size of the vagina and vulva is of little importance, since if they are too narrow, an incision along one side of the vagina passing deeply into the ischio-rectal space, and dividing the vulva widely, near the fourchette, will give all the room which is requisite. On the other hand a want of space between the ischia, giving a narrow outlet to the bony pelvis, is a serious contra-indication to the vaginal operation. In such a case and also when the tumor is five inches or more in diameter, abdominal hysterectomy is preferable.

When there is reason to think that the myoma is seriously complicated by pyo-salpinx, and that there are extensive intestinal adhesions, it is more difficult to decide which way is the best, but if the operator is not so sure of himself when operating from below, as when performing abdominal section, as will be the case with most of us here for some time to come, he will be apt to prefer the abdominal operation.

When the diagnosis is doubtful, and the tumor may, perhaps, be a dermoid cyst or a malignant growth, or when there is any reason why careful inspection of the pelvic viscera is requisite, abdominal section is preferable.

When the operation is performed for disease of the adnexa or for pelvic suppuration, there is a pretty general agreement that the vaginal operation is preferable in the very worst cases, *i. e.*, in those in which there are large collections of pus, from tubo-ovarian abscesses or from suppurating hæmatocele, where the pus is roofed over by dense adhesions of bowel and omentum, and the patient is feeble from absorption of septic principles and from want of food, etc. The ease and safety with which the pus may be evacuated in these cases, the thoroughness of the drainage, and the completeness of the recovery are really surprising.

When one has learned to trust the vaginal operation for the gravest cases of pyo-salpinx, it is easy to conclude to use it for less severe cases, and since the uterus is almost always diseased in cases of pyo-salpinx it is certainly best to remove it if it can be done, without increasing the danger too much, so that the complete vaginal operation seems indicated as being safer and better than complete abdominal extirpation.

When, however, there is apparently no pus present, and the operation is for pain and general impairment of health and loss of ability to work, when the uterus itself is little involved or not all, and it is probable that the appendages are densely bound down by old adhesions, I am not yet convinced that the vaginal operation is preferable.

Great friability of the uterus from malignant disease of the corpus and especially from recent pregnancy is a contra-indication of vaginal hysterectomy.

When it is not certain that both appendages are involved and it would be wrong to sacrifice the uterus if one tube could be saved, the vaginal vault can be opened behind, or better in front, the appendages examined and inspected. If one is diseased it can be removed in this way, while if the conditions warrant the removal of the other appendage extirpation of the uterus can then be performed.

In every case we must remember that exact diagnosis, manipulations controlled by sight and ability to meet and overcome unforeseen complications are the precious privileges of abdominal section by recent methods and with the pelvis elevated. In every case the operator must reflect as to how far he is willing to forego these obvious advantages in order to obtain those inherent in the vaginal operation, viz.: diminution of shock to the general system, and especially of the shock to the intestines, and freedom from abdominal wound and scar and from the liability to hernia.

DR. W. T. LUSK.—It is a matter of great interest to hear the experience of Dr. Polk, who has been the pioneer in this work in New York city. We are indebted very greatly, too, to Dr. Cushing for the demonstration of the methods employed.

I think it might be of some interest to go over the steps of my own conversion, for I am a thorough convert to this method. I was, however, like many others when, in 1892, at the meeting in Brussels, of the International

Society of Gynecology, the proposition was argued to substitute the vaginal route for most of the work that had been heretofore done by the abdominal method. It seemed to me at that time that we already had an excellent method, that our results could not be criticised, and that we had reached a stage when the slight mortality statistics had ceased to be regarded with any special wonder. Our thoughts were turning to a very important question—whether so many operations were needful, whether, indeed, a great many patients could not be cured without operation. The question was also arising as to how far the conservative methods of Polk, and of Martin, of Berlin, could be advantageously extended. When, therefore, it was proposed to interject a new question—whether the vaginal or abdominal route was preferable—I must say I received the suggestion with much impatience. However, we cannot dispose of such matters by a wave of the hand. My attention was drawn to the subject, and I was impressed by the wonderful rapidity of recovery in cases where the vaginal operation had been done. Then I had the opportunity of seeing the work of Péan, Pozzi, Ségond, Richelot, Jacobs, and others. I saw Ségond remove the uterus as large as the pregnant uterus at full term, by morcellation through the vagina in less than an hour, with the most perfect coolness, and with slight disturbance to the patient. This showed, not only the advantages of the pelvic route, but also that the method was capable of almost indefinite extension.

Since witnessing these operators I have been employing the method very largely. In removing fibroids by morcellation we find that, nothwithstanding the length of time in some cases employed, there is scarcely any appreciable effect upon the pulse and condition of the patient. Then came the cases of double pyosalpinx where the uterus was removed first, and the tubes afterward. Certainly, in extensive bilateral pus sacs this method was much safer than when an incision is made through the abdominal wall. The removal of small ovarian cysts and of small cysts of the broad ligament—from the size of an orange to that of a child's head—can be accomplished, I might say, entirely without risk or without shock. I hesitated longest regarding those cases where we hope at the outset that we may be able to save the tubes, the

ovaries, or parts of the ovaries. It seemed to me that conservative surgery could be better carried out through the abdominal incision, and I still think so where the posterior route is employed; but, according to Martin, if we use the Makenrodt incision—which furnishes a pretty large opening in front—it is possible in most of these cases, if the tubes are not excessively distended, to detach the tubes; and then to bring out first one cornu, then the fundus, then the other cornu, and then the tubes, and examine them at leisure. If tapping only is necessary, this can be done, or if more radical measures are required, these can be adopted with equal facility, and carried out under the guidance of the eye. The shock is not greater than that which ordinarily follows amputation of the cervix. I am sure, therefore, that these are not only coming methods on the grounds of their evident advantages, but there is another reason, being that the women themselves now insist upon having the vaginal route adopted. This applies even to the poor women coming to our public hospitals. This has been the experience in Paris and Berlin. But on its merits alone, I would say that the method is most desirable, when practicable. However, in making the change from the abdominal to the vaginal route, it seems to me that the individual operator should go slowly. When the method has been learned it is not more difficult than the older plan—I think it even easier—but one should be a little careful in employing the vaginal method to adopt it first only in the simpler cases, and gradually grow up to the technique, gradually learn the advantages and limitations of the method. I believe if this is done, the mortality will be fully as favorable as has been obtained by abdominal section. I am sure that these vaginal operations will be followed by much less discomfort to the patient. I hope the question will soon be settled, and that we shall before long be found discussing the most important question of all—in how many cases can we save these organs to the woman? We should remember that a woman who has had her ovaries and tubes resected is no more cured than a man when he has had his leg cut off.

DR. J. M. BALDY, of Philadelphia.—I felt that in coming here I was in a rather uncongenial atmosphere, and I feel all the more so since I have heard the papers, and also the remarks of your distinguished fellow-member,

Dr. Lusk. I am not in sympathy with the views advanced here to-night, and I am afraid I am almost alone in opposition to the vaginal operation. I have no quarrel with the good points of the vaginal method; I do not question for an instant many of the advantages and many of the results obtained in operations by this method; but I do think too little comparison is made between this and the abdominal route—the justly called "American procedure," in contra-distinction to the vaginal or "French method." But it is only by such comparison, it seems to me, that we can draw any real conclusions regarding the value of these respective methods. It is true that there is little shock by the vaginal method, but I insist it is just as true that there is as little shock by the abdominal method in the same class of cases, and I insist further that the operator that can do a given operation in a given case by the vaginal method can do it infinitely better and with less risk to the patient by the abdominal route. We are all familiar with the old theories and prejudices against opening the abdominal cavity on account of wounding the peritonæum. This same objection is again offered by the vaginal operator, and he tells me that by the vaginal method the peritoneal cavity is not opened. By the vaginal method, where we are dealing with inflammatory adhesions—and this forms the vast majority of cases—we do open the peritoneal cavity. I dare say, the statement that that cavity is not opened is a very telling reason for the vaginal operation to those who are not doing much of this class of work, but those familiar with such work will recognize that the statement is not founded on fact; there is not one case in one hundred by the vaginal method in which the peritoneal cavity is not opened as much as it would be by the abdominal section. We hear a great deal about adhesions, and about the intestines and the walls of lymph plastering over the pelvic cavity; but as a matter of fact we rarely find a case so complicated, except one class of cases—those in which there is a large pelvic intra-peritoneal abscess—which class I would exclude in this consideration. I think, therefore, that this argument, which is continually pressed forward in favor of the vaginal method, falls to the ground.

The question of being able to do conservative work on the appendages by the vaginal method is another point

frequently raised. I would ask by what method can we best do conservative work? I contend that it is infinitely more reliable with the open abdomen and the patient in the Trendelenburg position, where you have both sight and touch to aid you; that it is infinitely easier to make your diagnosis and settle the question whether or not the case is one in which the operation should proceed, or should end as an exploratory operation. By the vaginal method you must depend almost entirely upon touch; by the abdominal method you have both sight and touch to aid you.

The question of shock is continually brought up. Shock is not a great element in abdominal surgery. I am referring to pelvic surgery. I cannot recall having seen more than half a dozen cases of real shock in my whole experience in pelvic surgery, and most of these were either advanced cases of cancer or septicemia, in which the patients were in very bad condition, and some of them cases in which the operation could never have been completed by the vaginal method. Of course we see cases where the pulse becomes weak after the withdrawal of the ether, but this is not true shock; it is merely a diminution in the heart action due to the withdrawal of the ether.

When we come to consider the complications, we are brought face to face with a serious condition of affairs. You have heard the bright side of the vaginal operation. Now look for a moment at the other side—the side upon which the vaginal operator is not fond of dwelling, and to which he seldom refers except in the most general way. Hernia is brought forward very prominently as one argument against abdominal operations. I submit that hernia is becoming a very rare complication in comparison with what it used to be, and just so far as the abdomen is closed by proper methods, just so will it become less and less frequent. As a matter of fact, hernia also complicates vaginal operations, and not a small number of cases of this character are rapidly being added to the record. Suppose that a vaginal hernia follows the vaginal operation, and an abdominal hernia follows the abdominal operation, I would ask any surgeon which he would prefer to deal with. I can imagine no class of herniæ so difficult to treat as those following the vaginal operation. The abdominal herniæ are perfectly amenable to proper treatment, if taken in

time. It is true that we do not have as many herniæ following vaginal operations as after the abdominal operations, but it should be remembered that comparatively few vaginal operations have yet been done in this country.

What can be said in regard to fistulæ? It has been acknowledged by both the authors to-night that fistulæ do occur. There are fistulæ not only of the bowel, but of the bladder, following these operations. The proportion of fistulæ that have been reported by those doing vaginal hysterectomies abroad is simply alarming. There is hardly an operator who does not include fifteen or twenty fistulæ in his list of operations, and hardly one of these gentlemen that does not speak of the difficulty of closing some of them, and the almost impossibility of closing others. Most operators state that an abdominal operation is necessary to close the resulting fistula; therefore, it behooves us to consider this matter very seriously. Fistulæ of the bladder and of the bowel allowed to become chronic conditions are not injuries which we may deal with lightly. A secondary operation is infinitely more difficult than the original one. It is asserted that these fistulæ are rare, but I do not think that they are as rare as has been claimed. These accidents are of rare occurrence by the abdominal operation, and when they do occur can be immediately corrected.

If the ureter be injured, what is the result? Is the operator always cognizant of the fact that he has injured the ureter? As a matter of fact, such injury may often be done without the operator knowing it. If it has been done, an abdominal operation is absolutely necessary to repair this injury. Only recently I tore off a ureter in an operation, and only discovered this fact near the close of an abdominal operation. Three times in my experience the ureter has been torn or cut unavoidably, and in all of these cases the injury would never have been detected if the operation had been a vaginal one.

Dr. Polk has stated that adhesions above the true pelvis contra-indicate the vaginal operation. How many times do we find most of the adhesions in the upper part of the pelvis or in the abdominal cavity itself? Rarely.

I have never had the fortune to see a vaginal hysterectomist do a complete operation. I would like to refer to a case to which Dr. Polk himself referred before our Obstet-

rical Society in Philadelphia in the past few weeks. We frequently discuss these subjects, and argue them without standing on common ground, and we are, therefore, judging from different standpoints. The case referred to was one in which Dr. Polk operated in my presence. It was an ordinary case of pelvic inflammatory disease with adherent Fallopian tubes and ovaries, and no pus in either tube. The right side was freed and the fimbriated extremity found to be patulous, and it was decided that it was not necessary to remove it. The other appendage was closely adherent to the bowel, its fimbriated end was destroyed, and the tube in such a condition that it was considered advisable to remove the appendage on that side. An attempt was made to remove the left tube and part of this tube was removed. An unsuccessful attempt was made to remove the balance. From my standpoint I consider that an incomplete operation.

Dr. Polk, in speaking of this case, said that as no ligature was placed on that side, he did not consider it an incomplete operation.

DR. POLK (interrupting).—The ovary and tube on the affected side were removed with the exception of about half an inch of the tube.

DR. BALDY.—Supposing this to be the case, I still consider it an uncompleted operation. If this is accepted as a complete operation, then, of course, we are speaking on this subject from different points of view. If this operation had been done by Dr. Polk from above, the balance of the tube could have been removed and the patient could have been in bed from a half to three quarters of an hour sooner than she was. It has been my contention that the length of time that an expert operator will require to do the vaginal operation is almost twice as long as that required to do the operation by the abdomen. This prolongation of an operation, and, incidentally, of the etherization, should be taken into account. I believe the majority of French surgeons quote from ten to twenty per cent. of incomplete operations.

Convalescence has been stated to be quicker after the vaginal operation, but to this I must take exception; the difference again is in the point of view. An abdominal operator believes that his patient is better off in bed for

four weeks, irrespective of her recovery surgically; but the vaginal hysterectomist allows his patient up as soon as she is surgically well.

It seems to me that when we consider all these facts, and make a comparison between the possible accidents and the results, the balance is altogether in favor of the abdominal operation, sentiment not considered. The abdominal wound does not seem to me to be a serious matter if we can avoid the occurrence of hernia.

DR. E. B. CRAGIN.—The first vaginal cœliotomy I ever saw was performed by Dr. Polk, and the more I operate, the more thankful I am that he started me in doing vaginal cœliotomies. The more I see of vaginal work, the more pleased I am with it. It seems to me that in comparing the two operations we must look at the condition of the patient at the time of the operation, and also at the ultimate results of the operation. Shock has been referred to, but from what has been said, there must be shock and shock, for all who operate through the abdomen must have noticed that the patients are depressed, even if this be not called "shock." I think we must all have noticed that this depression is not as great after the vaginal operation as after the abdominal. Doctor Baldy has spoken of excluding from this consideration large pelvic intra-peritoneal abscesses, but these are exactly the cases where vaginal operation has the advantage over the abdominal. We must not exclude them—these pus cases. By doing the vaginal operation we avoid the sepsis and the shock that would be more likely to occur by operating from above, and the woman recovers more easily.

I find that since March 2, 1895, I have done 55 vaginal cœliotomies and 53 abdominal, showing that the two operations have been about equally divided. Of the 55 operated upon through the vagina, 42 were diseased appendages, while only eighteen cases were operated upon from above for diseased appendages. Of these eighteen, a large proportion were for large ovarian cysts. My experience, therefore, is that about 75 per cent. of cases of inflammatory conditions of the appendages are suitable to be operated upon from below, and 25 per cent. from above. I think that the more we operate from below, the better pleased we are with it.

I believe that there is a little more danger of injuring

the rectum in the vaginal operation, than in the abdominal, and yet there are dangers connected with an abdominal operation. Dr. Baldy has just admitted that he has seen three injuries to the ureter in abdominal operations. If the uterus is removed, most of the fæcal fistulæ close spontaneously. If we compare the results of the two operations and the complications—the fæcal fistulæ with ragged edges, done from below and from above—most of us will admit that the mortality would be large in cases operated upon from above. I believe the vaginal operation has come to stay, although we may limit the field more in the future. We should be very careful to classify and select our cases. I think the cases operated upon by the vaginal method are better, not only immediately after the operation, but six months afterward. I, therefore, believe that vaginal cœliotomy is an operation which has a future.

DR. C. P. NOBLE, of Philadelphia.—Usually young men are accused of running after new things, and the older ones are supposed to be more conservative, but to-night I believe this position is reversed. We have heard the favorable points for the vaginal operation, but everything has been in very general terms. I should like to ask Dr. Polk and Dr. Cushing how many vaginal operations they have done, how many deaths they have had, and how many fistulæ they have had.

DR. POLK.—The mortality has been 3 in 72, and the fistulæ 2 in 73.

DR. CUSHING.—I cannot at this moment give the exact figures, but I have had about 75 vaginal hysterectomies with 5 deaths. There have been 2 urethral fistulæ, and 1 bladder fistula, all of which I have cured. More recently, there have been 20 cases with 1 fatality. All the operations have been complete.

DR. NOBLE.—Dr. Baldy has covered a good deal of ground I had intended to go over. Much depends upon the morbidity, the sequelæ, the convalescence, and the ability to deal with complications, and these points should determine our choice of a method. At the present time, I think there can be no question that the mortality in all cases is lower by the abdominal route, but in the hands of good operators, the mortality by both methods is so low

that we cannot quarrel over that. Last May, in Baltimore, Jacobs reported a large series of cases with a mortality of 4.2 per cent. from vaginal hysterectomy, and Baldy a series of hysterectomies done by the vagina, with a mortality of 2.7 per cent. Dr. Kelly has had 107 abdominal hysterectomies for inflammatory conditions without a death. I think the mortality depends chiefly upon the operator. It is alleged by those who advocate the vaginal operation that there is much poor health after abdominal operations, but judging from my own experience this is not the case. Five per cent. would certainly cover this morbidity in my own cases. I think adhesions occur under both methods of operating. The vaginal hysterectomist claims that he does not break up adhesions in many cases, and therefore, more adhesions must exist after this method of operation. Most operators per vaginam have done incomplete operations in about ten per cent. of their cases, and this must be contrasted with the small number of infected pedicles after the abdominal method. So far as my own experience in abdominal surgery goes, I have but twice opened the bowel. In one of these cases it was sutured at once, and there was no fistula. I think there can be no question that bowel and bladder fistulæ are much less frequent by the abdominal route, and the abdominal operator can at once close the rent if such an injury should occur.

The rapidity of convalescence seems to me to be much exaggerated. My own cases of vaginal hysterectomy have been in the hospital as long as the abdominal hysterectomies. Most of them smelled so badly at the end of a week that I would have been glad to have sent them home at that time had they been well enough. In my opinion, the convalescence is much more rapid after the abdominal operation. Most of these patients are broken down women, and several weeks of rest in bed is a valuable factor in restoring their general health, and hence the matter of the time spent in bed is a trifling one.

There seems to be no question that the abdominal route offers very great advantages regarding the completeness of the operation. All my abdominal operations have been completed with one exception. That case was one of abscesses disseminated throughout the abdominal cavity, and the abdominal viscera could not be differentiated.

The abdominal route offers infinitely greater advantages than the vaginal route in the matter of the facility with which complications arising during the operation can be dealt with. The same may be said of the ability to control hemorrhage in the abdominal operation. I have seen recently two cases of appendicitis with abscess develop shortly after labor. I saw each case about three weeks after labor. In each the abscess was in the false pelvis. If they had been operated upon by the vaginal method it would have been necessary to remove the uterus before the true condition could have been properly treated.

The great argument against the abdominal operation has been the frequency with which it is followed by hernia. A few weeks ago I reported my own work in suturing the abdominal wall with buried silk-worm gut. Two hundred cases have been sutured in this way, and so far as I have been able to ascertain there has not been a single relapse; hence, I think the argument against the abdominal operation producing hernia has but little force.

If I had to operate in a hospital where the facilities for asepsis were poor, I should certainly operate from below. It seems to me very crude surgery to go back to the old method of securing bleeding by clamps. This produces sloughing, and is like tying a string around the bleeding finger, and allowing it to slough off.

In my judgment, large pus collections in the pelvis should not be dealt with by either abdominal or vaginal hysterectomy. They should be simply incised through the vagina, and drained. If necessary, a second operation can be done. I have frequently removed fibroids in from seven to ten minutes, and hence, I cannot understand why Dr. Lusk was so delighted to see fibroids removed in an hour and a half!

DR. LUSK (interrupting).—I meant to say that even if it took one and a half hours to remove a fibroid by vaginal hysterectomy the patient showed scarcely any shock.

DR. NOBLE.—I am prepared to believe that anterior colpotomy will enable us to remove small fibroids, but where they are of any size rather than deal with them by morcellation, I would certainly resort to abdominal section.

In operating for tubal pregnancy I think the advantage is distinctly with the abdominal route except in the small

number of cases where there is a suppurating hæmatocele. The cases that have died in my hands have been those in which the abdomen was filled with blood at the time of operation. Every case where the tube has been unruptured, or the blood walled in, has recovered. If we operate from below in such cases, I am sure that we shall sacrifice unnecessarily many ovaries. In the milder inflammatory cases there seems to be a decided advantage in operating from above because it seems to offer a much better chance for careful resection of the tubes and ovaries. I very frequently stitch up a lacerated perineum, and then do an abdominal operation at the same time, thus saving the patient a second operation. This cannot be done after vaginal hysterectomy.

Recently, through the studies of Dr. Kelly regarding catheterization of the ureters, Dr. Clark has been led to make an important suggestion regarding abdominal cancer. If the bougies be introduced into the ureters, not only can the tubes, ovaries, and uterus be removed, but also the entire broad ligament. This is certainly a much more thorough mode of operating than by the vaginal route.

I do not wish it to be understood that we should do no operations through the vagina. I think large pelvic abscesses should be operated upon through the vagina, because the mortality by the abdominal route is considerable. There is also an advantage in operating from below on very fat women. Anterior colpotomy promises well also for some few conditions.

DR. H. J. BOLDT.—In comparing the relative value between an abdominal and a vaginal section it is necessary to consider the pathological conditions for which such section is made. My remarks are based upon a sufficiently large experience with both methods of operation to justify me in coming to some definite conclusion regarding the merits of these operations for certain pathological lesions. We can remove ovarian tumors, even of considerable size, per vaginam, provided that the neoplasms are movable. Again, we have by means of the vaginal operation a method of breaking up the adhesions of a fixed retro-position of the uterus which gives equally as good a result as the abdominal incision. For tubal gestation a vaginal operation may be

substituted for the abdominal procedure in such cases where it is apparent that no rupture has taken place, or that subsequent to the rupture the hemorrhage has ceased. Myo-fibromatous tumors of the uterus can be readily attacked per vaginam, if they are not large, but I would limit their size to such as do not extend more than three or four fingers' width above the symphisis with the proviso that the vaginal canal should be sufficiently roomy to allow one to work with ease. The variety of tumors to which the operation from below is applicable are the submucous and the interstitial tumors, as well as those of the vaginal portion; subserous tumors on the anterior surface of the cervix may also be readily removed from below. For all other varieties of tumors of this nature and for the large size tumors, as well as for neoplasms occurring in virgins (unless they are readily accessible per vaginam) I deem abdominal hysterectomy by far the most preferable method.

We have now to consider the most interesting class of cases, namely, those patients who have multiple pelvic abscesses with or without suppurative diseases of the fallopian tubes, and cases of recurring pelvic peritonitis with tubo-ovarian disease, chronic metritis, and endometritis complicated with pelvic inflammation which will not yield to ordinary methods of treatment. For all such cases the vaginal method of operating is decidedly that which yields the best results in the hands of most operators of experience, if we consider both the immediate and remote terminations combined, although there are exceptions in which it may become necessary to also open the abdomen, thus making the combined operation necessary, namely, when intestines are injured during a vaginal operation; which is, however, a very rare accident in the hands of those accustomed to this kind of work. The really complicated cases of pelvic abscesses are, as a rule, incurable except by a radical operation, that is by removal of the pelvic organs, yet if such undertaking is resorted to per abdominal section the prognosis is exceedingly unfavorable, whereas, if one operates from below, both the direct and ultimate result is almost invariably favorable. It is, of course, desirable to remove all pathological conditions, including the adnexa, but it is by no means absolutely necessary to succeed in this in order to obtain a perfect

cure. The reasons for this I am unable to give; however, the facts are proven by experience that such is the case, not only in my own cases, but those of every operator whose experience I have seen in print; it is possible, however, to remove all pathological products in nearly all instances with patience and careful work.

To say that a vaginal operation is to be invariably preferred over abdominal section is, in my estimation, an error, because there are cases in which it is evident that one uterine appendage can be saved by conservative surgery, and I deny the possibility of being equally conservative with operations done per vaginam as one can be in operating from above, limiting this restriction to the complicated cases. It is also my conviction that a more satisfactory diagnosis can be made if an abdominal section is done, provided the operator makes his incision sufficiently long to utilize his eyesight as an aid for diagnosis. It is not necessary for me to dwell in detail upon the obvious great advantages of a vaginal operation over the abdominal operation, as far as convalescence is concerned; the dangers of subsequent hernia, and the risks encountered from intestinal adhesions to the parietal peritoneum at a subsequent period. I do not believe that the risk of injuring the ureters or the intestines is any greater if one operates per vaginam than if operated per abdomen.

DR. H. N. VINEBERG.—I believe I was the first to introduce the operation of vaginal fixation, which was the forerunner of vaginal section for conservative work. My first vaginal section for conservative work was done in November, 1894, and several cases were reported in the *Medical Record* for March, 1895. It is in this class of cases that I consider the vaginal method so valuable. I do not agree with those who say that a portion of an ovary cannot be resected as well through the vagina as through the abdomen, for my experience is entirely opposed to this view. Another advantage of the method is that in the cases where there is more or less disease of the adnexa there is usually retroversion of the uterus, and this retroversion can be treated at the same time. The uterus is brought forward and sewed to the vaginal wall. I am glad to see that the anterior vaginal incision is meeting with favor. It is the one which I first advocated, and I still adhere to it. In the majority of cases you can do almost all that is re-

quired through this incision. I cannot agree with the reader of the paper that it is necessary in these cases to do an Alexander's operation. I cannot see that there is any advantage in fixing the uterus by the round ligaments over the fixation to the vaginal wall. I was rather surprised at one remark by Dr. Noble, i. e., that in doing vaginal work perineorrhaphy could not be done at the same time. This is certainly not the case; you can do several of these operations at one time with the greatest facility. I believe in almost every case, as one speaker has said, that the peritoneal cavity is opened, even by the vaginal method.

DR. P. F. MUNDÉ.—I think it is about time that some of the gentlemen in New York city said something about the advantages of the abdominal method, otherwise it will appear that this method has been entirely abandoned here. I do not believe that the impression should be allowed to go forth that we do everything per vaginam, simply because quite recently there has been a sudden enthusiasm awakened in regard to operating by the vaginal route. I take it that the removal of the uterus and the appendages is the point most in question. As far as my experience is concerned, I would say that I have removed the uterus per vaginam for cancer in twenty-seven cases with twenty-four recoveries, but I shall never do it again because I have done it by the supra-pubic method much more easily, and with quite as good results for the patient. In every one of the twenty-four recoveries recurrence took place, and the same may be said of those treated by the supra-pubic method. There was certainly no more shock by the abdominal method, and it was in every way as satisfactory as the vaginal operation.

Regarding the removal of fibroid tumors, I would say I have removed only thirty-three by the abdominal hysterectomy. Of these, four died. When I compare the two methods, and consider the difficulty of controlling hemorrhage, and the number of instruments required in the field of operation, the vaginal route appears to me like night, and the abdominal route like day.

I have always said that for fluid accumulations, whether of pus, blood, or serum, which could be removed by the vagina, I should always operate from below. I was severely criticised for this position some years ago, but many have

since come around to my way of thinking. Where there are numerous abscesses, and the vagina is roomy, and we can open these sacs and drain them, with or without removing the uterus and appendages, the vaginal route is suitable, and it is to this class that I would limit the vaginal operation. I should not remove ovarian tumors or cysts or fibroids through the vagina, unless they were small, or particularly accessible by this route. I do not believe the majority of pelvic surgeons in this city are in favor of the vaginal route exclusively.

DR. E. E. TULL.—In answer to the question regarding the completeness of the vaginal operation, I would like to present a specimen removed through the vagina in twelve minutes. Convalescence was quick and easy. The specimen speaks for itself. I have done the operation in fifty cases with two deaths. Both of the fatal cases were due to pure accidents; namely: in one case the nurse removed the clamp an hour after the operation because the handle pressed against the patient's leg. In the second case the patient sat up six hours after operation and tore the clamp off. I cannot understand why it is necessary to use so many clamps and ligatures. I removed recently a ten-pound tumor with two clamps and two ligatures. The clamp should rarely be used except to bring the upper border of the broad ligament in apposition.

DR. W. GILL WYLIE.—My experience has been very largely with the abdominal method—about 1,500 abdominal operations as compared with 100 vaginal operations. In my first hundred laparotomies for diseased tubes and ovaries I lost only five patients, and this was ten or twelve years ago. Out of 100 vaginal hysterectomies, only one patient has died, but this is no better than my mortality from the abdominal method in this same class of cases. In my private hospital, one per cent. would cover all classes of cases. There has been so much more work in this country by the abdominal method that we cannot yet fairly compare the results by the two methods. If I had to employ only one method, it would certainly be the abdominal method, for it is applicable to the larger number of cases. I am satisfied that my vaginal hysterectomies have been successful because they have been done in a favorable class of cases. I am satisfied that if the great

surgeons of France had been as expert in abdominal surgery as were the surgeons of England and Germany, the vaginal method would not have been so strenuously advocated.

DR. POLK.—We are to be congratulated on the spirit which has been displayed in the remarks of the various speakers to-night, and we are particularly indebted to our friends from Philadelphia for the candor with which they have spoken. The Academy of Medicine is evidently a catholic institution; it is so broad in all its views that it can include within its walls all shades of opinions. It is our pride that here every one rises to speak about his results as he, and he alone, sees them. The differences which have been brought out have been so clearly stated that it would be mere repetition to go over that ground again, but a few points should receive attention.

There seems to be but little difference of opinion regarding which is the easier route—undoubtedly the abdominal section is far easier, even though by the vaginal method the parts can be brought well outside of the body. We are oftentimes led to a species of indolence by the ease with which we accomplish our purposes, and are led to close our eyes to methods which demand special effort on our part, when we have already secured by another method all that seems demanded. But the easier operation is not always the better operation for the patient. As a general rule, it may be said that the easier the operator can do his work, the better that work will be done, but if in attaining this ease of operating, he must do violence to important structures, this proposition cannot be considered as always holding good. We have an exception here. I would suggest this as a possible answer to some of the criticisms made on the two methods. Dr. Baldy seemed to think that the advocates of the vaginal method were shy of entering the peritoneal cavity, but in this he was mistaken, for we are certainly as likely to enter the peritoneum from below as from above. Dr. Noble seemed surprised that any one would advocate the removal of a fibroid per vaginam on account of the slowness of the method. I would like to ask if the five or seven minutes occupied in the operation included the removal of the uterus, and the completion of the operation up to the closure of the abdominal wound?

DR. NOBLE.—At the end of the five or seven minutes the patient was as much closed up as she would be at the completion of the operation when done from below. Probably about fifteen minutes were consumed in the suturing, to put the parts in condition for immediate healing.

DR. POLK (after interruption by Dr. Noble).—It is evident after all that considerably more than seven minutes is required to fully complete the operation. There is apparently no such vast difference in the time consumed in operating by the two methods.

I made no mention of cancer in my paper because I was in doubt as to whether vaginal section was really the better way of treating this disorder. This question was discussed about a year ago in the New York Obstetrical Society by Dr. Cragin and myself in regard to two cases which I had operated upon. It seems to me that the time has come when, in cases of cancer, we can go back to the operation of Freund and add to it the removal of the broad ligament and such infected glands as may exist. This is a procedure which must be done by the abdominal route.

I trust I may not be considered lacking in courtesy to any of the participants in this discussion when I say that none has grasped the great underlying principle. If surgery means anything it means the cure of the patient by operation, and if that cure is to be accomplished, it goes without saying that the quicker the diseased structure can be submitted to operation, the better will be the result. The discussion has been carried on upon the line of advanced disease, when the conditions are such in most instances as to force the surgeon into an operation. But the great underlying principle is that pelvic disease begins in the pelvis, and generally in the bottom of the pelvis in close relation with the vagina. If this structure be incised, the risk to life is so insignificant that it may be ignored. In cases of salpingitis following gonorrhœal infection, for example, we know that the integrity of many pelvic structures is threatened, and we know that early incision of the cul-de-sac in these cases will save those appendages from this destructive inflammation. Again, if we know anything, we know that if these incisions are made into the vagina even at a late stage, the result will be not only temporary relief, but a tendency to save these structures. The same argument applies to the other diseased condi-

tions touched upon in this discussion. Every one interested in pelvic surgery must take into consideration the advisability of resorting to vaginal section at the outset. Having once adopted this practice, you will have wiped out most of the objections which have been stumbling-blocks to-night.

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[1] For paper see page 266.

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SCIENTIFIC WORK

IN THE

SECTIONS OF THE ACADEMY.

JANUARY 15TH, 1895.—R. T. Morris, M. D. Paper: "The etiology of appendicitis." Published in author's "Lectures on Appendicitis," 2d ed. A large number of specimens were exhibited in illustration. Discussion by Drs. A. M. Phelps, T. H. Manley, L. F. Bishop, S. H. Dessau, and W. H. Porter.

MARCH 19TH, 1895.—L. F. Bishop, M. D. Paper: "A review of the present state of our knowledge of gout, and a discussion of its comparative frequency in America." Published: Med. Rec., 1895, xlvii., 454. Discussion by Drs. W. H. Draper, F. P. Kinnicutt, H. D. Chapin, and W. H. Porter.

APRIL 16TH, 1895.—A discussion on the diagnosis of tumors of the gastro-intestinal tract without and with the aid of exploratory laparotomy was introduced by Willy Meyer, M. D., who also presented a number of illustrative specimens. Discussion continued by Drs. B. F. Curtis, B. Robinson, E. G. Janeway, M. Einhorn, and R. T. Morris.

MAY 21ST, 1895.—Exhibition of an electrical heating apparatus, the "Electrotherm."

M. Einhorn, M. D. Paper: "A further report on achylia gastrica." Published: Med. Rec., 1895, xlviii., 5. Discussion by Dr. L. F. Bishop, and F. B. Turck, of Chicago.

L. P. Clark, M. D. Paper: "The Flechsig method in the treatment of insane epileptics." Published: Amer. Med. Surg. Bulletin, 1895, viii., 708. Discussion by Drs. F. Peterson, E. D. Fisher, J. Collins, and T. H. Manley.

Demonstration of instruments employed in examining and treating the diseases of the stomach by Fenton B. Turck, M. D. Published: Amer. Med. Surg. Bul., 1895, viii., 772.

OCTOBER 15TH.—J. B. Mattison. Paper: "Morphinism in the old. Cases at three score and ten: Recovery." Published: Atlantic Med. Weekly, 1896, v., 355.

Prof. L. H. Friedburg, M. D. Paper, on "Hæmogallol, and the assimilation of iron by the organism." Published: Amer. Med. Surg. Bul., 1895, viii., 1230.

Dr. W. H. Porter reported his experience with this drug.

NOVEMBER. Section in charge of a general meeting of the Academy.

DECEMBER 17TH, 1895.—C. E. Nammack, M. D., presented a case of splenic myelogenous leuchæmia not cured by bone marrow.

T. E. Southworth, M. D., exhibited fresh and strained microscopical preparations of blood from Dr. Nammack's case. M. Mango, M. D., discussed the case and its treatment.

R. C. Newton, M. D., reported a case of lieno-medullary leuchæmia.

E. E. Smith, Ph. D. Paper: "Notes on urinary analysis. *a*. Methods used by the writer for detecting traces of albumin in the urine; *b*. Standards in quantitative urinary analysis." Published: Med. Rec. 1896, xlix., 13. Discussion by Drs. M. Mango, and B. Symonds.

WILLIAM G. LEBOUTILLIER,
Secretary.

SECTION ON GENERAL SURGERY.

JANUARY 14th, 1895.—*Cases presented:*

1. Robert Abbe, M. D. Three cases of extrophy of bladder in infants.
2. J. E. Nichols, M. D. Reproduction of tibia after necrosis.
3. J. A. Blake, M. D. Reproduction of portion of humerus.
4. A. G. Gerster, M. D. Excision of the knee in the adults for tuberculosis.

R. Guiteras, M. D. Paper: "The sub-gluteal triangle and its surgical importance." Published: Annals Surg., 1895, xxii., 311. Discussion by Drs. Woolsey, Dawbarn, Brockway, McGrath, and Guiteras.

FEBRUARY 21st, 1895.—In charge of a stated meeting of the Academy.

MARCH 11th, 1895.—*Cases presented:*

1. C. N. Dowd, M. D. Two cases of partial removal of the tongue for carcinoma by Whitehead operation.
2. F. Kammerer, M. D. Nephrectomy in a child for rupture of the kidney.
3. B. B. Gallaudet, M. D. Suture of the musculospiral nerve.
4. W. B. De Garmo, M. D. Five adults on whom he had performed the radical cure for hernia.
5. Royal Whitman, M. D. A child with a congenital deformity of the coccyx.

W. B. De Garmo, M. D. Paper: "A report of thirty-five cases of amputation of the omentum in hernia operations." Published: Annal. Surg., 1895, xxi., 658. Discussion by Drs. Curtis, White, Lilienthal, Milliken, Coley, and De Garmo.

APRIL 8th, 1895.—*Cases presented:*

1. B. F. Curtis, M. D. Plastic operation on the nose following carcinoma.
2. R. Whitman, M. D. Bending of the neck of the femur; operation.
3. W. B. Coley, M. D. Inguinal hernia, complicated by cystic tumor of the cord; operated.
4. R. F. Weir, M. D. "Saddle nose" treated with a celluloid support.
5. E. M. Cox, M. D. Compound comminuted fracture of patella; operation; complete recovery.

J. B. Walker, M. D. Paper: "The ectopic or undescended testis." Published: Amer. Med.-Surg. Bul., 1895, viii., 556. Discussion by Drs. Curtis, Lilienthal, De Garmo, Coley, Manley, and Walker.

R. H. Cunningham, M. D. Paper: "The preparation of aseptic catgut by means of formalin." Published: N. Y. Med. Jour., 1895, lxi., 494. Discussion by Drs. Curtis, Walker, Coley, and Lilienthal.

P. R. Bolton, M. D., presented a specimen showing the final result (four years after the operation) of an anastomosis of the ilium to the sigmoid flexure of the colon.

MAY 13th, 1895.—*Cases presented:*

1. A. J. McCosh, M. D. Thyroidectomy for opthalmic goitre.
2. B. B. Gallaudet, M. D. Suture of patella after fracture.
3. H. Lilienthal, M. D. Nephrectomy for renal calculus.
4. E. Eliot, M. D. Resection of buccal nerve for neuralgia.

G. Woolsey, M. D. Paper: "Repair after bone flap operation on the skull." Published: Med. Rec.,

1895, xlvii., 786. Discussion by Drs. Dawbarn, Fisher, and Curtis.

J. F. Erdmann, M. D. Paper: "The use of torsion instead of ligatures." Published: Med. Rec., 1895, xlvii., 805. Discussion by Drs. Curtis and Dawbarn.

OCTOBER 14th, 1895.—*Cases presented:*

1. B. F. Curtis, M. D. Nephrectomy for pyonephrosis.
2. A. M. Phelps, M. D. Congenital dislocation of shoulder.
3. F. J. Torek, M. D. Carcinoma of breast: Willy Meyer's method.

D. Bryson Delavan, M. D. Paper: "Recent advances in the surgical treatment of malignant disease of the larynx." Published: N. Y. Med. Jour., 1896, lxiv., 318. Discussion by Drs. Curtis and Delavan.

NOVEMBER 11th, 1895.—*Cases presented:*

1. C. N. Dowd, M. D. Epithelioma of the floor of the mouth one and a half years after operation.
2. Royal Whitman, M. D. Cases illustrating the cure of flat-foot by forcible over correction and subsequent support.

Willy Meyer, M. D. Paper: "Catheterism of the ureter in the male and female with the help of Casper's ureter-cystoscope." Published: N. Y. Med. Jour., 1896, lxiii., 369. Discussion by Drs. Bangs, Brown, Harris, and Meyer.

W. B. Coley, M. D. Paper: "The disadvantages of non-absorbable sutures in hernia operations." Published: N. Y. Med. Jour., 1896, lxiii., 275. Discussion by Drs. Crary, Cole, Curtis, Dowd, Walker, and Coley.

DECEMBER 9th, 1895.—*Presentation of Cases:*

1. J. A. Wyeth, M. D. Ligation of left subclavian (3d division) transversalis colli and suprascapular arteries under cocaine anæsthesia; subsequent bloodless amputation at the shoulder-joint.
2. C. L. Gibson, M. D. Reduction of old dislocation of shoulder.
3. T. H. Manley, M. D. *a*. Infant with congenital dislocation at knee. *b*. Adult with aneurism of the subclavian artery.
4. Willy Meyer, M. D. Supra-malleolar osteotomy, followed by over correction of the deformity and the application of Whitman's splint.

F. Tilden Brown, M. D. Paper: "Report of two successful hip-joint amputations, performed by new method." Published: Annal. Surg., 1896, xxiii., 153. Discussion by Drs. Dawbarn, Wyeth, and Brown.

G. E. Brewer, M. D. Paper: "Report of post-mortem examination of the parts involved in a Bassini's operation for inguinal hernia six weeks after the operation." Amer. Med.-Surg. Bul., 1896, ix., 280.

ROYAL WHITMAN, M. D.
Secretary.

SECTION ON GENITO-URINARY SURGERY.

JANUARY 8TH, 1895.—W. R. Pryor, M. D. Paper: "Latent gonorrhœa in women." Published: Jour. Cutan. and Gen.-Urin. Dis., 1895, xiii., 89.

FEBRUARY 12TH, 1895.—R. Guiteras, M. D. Paper: "The present treatment of urethritis in New York." Published: Med. News, 1895, lxvii., 365.

B. E. Vaughan, M. D. Paper: "Experiments in the treatment of syphilis by animal blood serum."

MARCH 12TH, 1895.—J. Vanderpoel, M. D. Paper: "Strangulation of the testis and epididymis from torsion of the spermatic cord." Published: Med. Rec., 1895, xlvii., 737.

E. Fuller, M. D. Paper: "Clinical symptoms due to chronic perivesical imflammation." Published: Med. Rec., 1895, xlvii., 461.

APRIL 9TH, 1895.—G. Woolsey, M. D. Paper: "Some considerations on prostatectomy." Published: Jour. Cutan. and Gen.-Urin. Dis., 1895, xiii., 280.

J. R. Hayden, M. D. Paper: "Double castration for hypertrophy of the prostate gland." Published: Med. Rec., 1895, xlvii., 612.

NOVEMBER 12TH, 1895.—W. R. Pryor, M. D. Paper: "The palliative treatment of gonorrhœic tubal disease." Published: Med. Rec., 1895, xlviii., 838.

F. A. Fort, M. D. of Paris, France. Paper: "Electroliser, for the surgical treatment of stricture." Published: N. Y. Med. Jour., 1895, lxii., 625.

DECEMBER 10TH, 1895.—C. L. Gibson, M. D., presented a case of "Tubercular epididymitis and orchitis."

R. Guiteras, M. D., presented a kidney removed by nephrectomy in a patient suffering from nephrolithiasis.

F. T. Brown, M. D., presented a headlight for urethral illumination; a perineal tube-holder; an irrigation tube with a guard; and a probe-pointed, grooved catheter.

F. T. Brown, M. D.—Paper: "The perfected metrourethrotome." Published: Am. Med. Surg. Bul., 1896, ix., 623.

W. K. OTIS,
Secretary.

SECTION ON ORTHOPÆDIC SURGERY.

JANUARY 18TH, 1895.—Discussion on the prognosis of hip disease.

FEBRUARY 15TH, 1895.—A. M. Phelps, M. D. Paper: "The treatment of deformities of the hand." Discussion by Dr. Gibney, Dr. Brackett, of Boston, Drs. Ketch, Sayre, Berg, Whitman, and Judson.

MARCH 15TH, 1895.—T. Halsted Myers, M. D. Paper: "The treatment of fractures of the lower extremity by apparatus designed to shorten the period of recumbency." Not published. Discussion by Drs. Shaffer, Sayre, Ketch, Berg, and Hubbard.

APRIL 19TH, 1895.—No paper. Clinical meeting.

MAY 17TH, 1895.—J. Teschner, M. D. Paper: "The treatment of postural deformities of the trunk by means of rapid and thorough physical development." Published: Annal. Surg., 1895, xxii., 226. Discussed by Dr. E. G. Brackett, of Boston, Drs. Wilson and Young, of Philadelphia, Dr. Lovett, of Boston, and Drs. Lillienthal, Reginald Sayre, and Ketch.

NOVEMBER 15TH, 1895.—R. H. Sayre, M. D. Paper: "The results of the removal of the astragalus contrasted with forcible manipulation." Published: Am. Med. Surg. Bul., 1895, viii., 1476. Discussion by Drs. Judson, Taylor, Whitman, Teschner, Sayre, Berg, Hubbard, and Townsend.

DECEMBER 20TH, 1895.—H. L. Taylor, M. D. Paper: "Congenital dislocation of the knee." Published: Tr. Am. Orthop. Assn., 1896, viii., 239. Discussion by Drs. Judson, Whitman, Sayre, Gibney, and Townsend.

H. W. BERG,
Secretary.

SECTION ON OPHTHALMOLOGY AND OTOLOGY.

JANUARY 21ST, 1895.—F. E. Dench, M. D. Paper: "Case of neuro-retinitis following scarlet fever."

W. B. Marple, M. D. Paper: "Contribution to the pathology of embolism of the central artery of the retina." Published: N. Y. Eye and Ear Infirmary Annual Reports, Vol. iii., 1895.

FEBRUARY 18TH, 1895.—W. A. Holden, M. D., presented a specimen of siderosis.

H. S. Oppenheimer, M. D. Paper: "Non-operative cure for insufficiency of the internal recti muscles." Published: Trans. Amer. Ophthal. Soc., 1895.

H. H. Seabrook, M. D. Paper: "Non-operative treatment of muscular insufficiency." Not published.

MARCH 18TH, 1895.—A. Duane, M. D. Paper: "The parallax test for heterophoria." Published: Archives Ophthalmol., 1895, xxiv., 258.

J. H. Claiborne, M. D. Paper: 1. "Case of hysterical amblyopia in both eyes with recovery in a few days." 2. "Case of homonymous hemianopsia with paralysis of left arm and left side of face." Published: N. Y. Polyclinic, 1895, v., 109.

P. Fridenberg, M. D., presented a case of exophthalmic goitre with monicular symptoms, similateral thyroid hypertrophy. Published: Graef's Archives, 1895, xli., p. 158.

APRIL 15TH, 1895.—J. E. Weeks, M. D. Paper: "Report of one hundred consecutive cataract extractions." Published: N. Y. Med. Journ., 1895, lxii., 137.

MAY 20TH, 1895.—H. E. Stafford, M. D., presented a case of anterior polar cataract.

D. Webster, M. D., presented a case of detachment of retina and intra-ocular growth.

W. B. Marple, M. D., presented a case of hyperopia of fifteen diopters.

W. B. Johnson, M. D. Paper: "The New Jersey state law in regard to ophthalmia neonatorum." Published: Ophthal. Record, 1895, v., 55.

OCTOBER 21ST, 1895.—Alice Wakefield, M. D., presented a case of corneal pigmentation resembling dislocated lens into anterior chamber.

W. A. Holden, M. D., presented a case of hæmorrhage into cornea.

W. B. Marple, M. D., presented a case of hysterical amblyopia in one eye.

C. J. Foote, M. D. Paper: "Bacteriology of the normal conjunctiva." Published: Med. Rec., 1896, xlix., 765.

F. M. Wilson, M. D. Paper: "Bacteria and ophthalmic surgery." Published: Med. Rec. 1896, xlix., 766.

NOVEMBER 18TH, 1895.—H. Knapp, M. D., presented a case of empyema of frontal sinus.

H. S. Oppenheimer, M. D., presented a case of hæmorrhage at macula.

F. W. Ring, M. D. Paper: "Alveolar sarcoma of optic nerve." Published: Archv. Ophthalmol., 1896, xxv., 51.

DECEMBER 16TH, 1895.—W. B. Marple, M. D., reported a case of peculiar deposit on Desermet's membrane.

D. Webster, M. D. Paper: "Recurring monocular hæmorrhage from disease of the heart." Published: Amer. Med. Rev., 1896, ii., 21.

W. H. Bates, M. D. Paper: "The Stacke operation for the cure of otorrhea." Published: Amer. Med. Surg. Bul., 1896, ix., 64.

W. B. MARPLE,
Secretary.

SECTION ON LARYNGOLOGY AND RHINOLOGY.

JANUARY 23D, 1895.—W. C. Phillips, M. D., exhibited a case of congenital occlusion of the vault of the pharynx.

J. E. H. Nichols, M. D., exhibited a case of extensive sarcoma of the nasopharynx.

T. J. Harris, M. D., and R. C. Myles, M. D. each, presented cases of empyæmia of the frontal and maxillary sinuses cured by operation.

D. B. Delavan, M. D. Paper: "The effects of deafness in others upon the child's voice." Published: Med. Surg. Bulletin, 1895, viii., 240.

R. C. Myles, M. D. Paper: "Diseases of the accessory nasal sinuses with suggestions regarding their treatment." Published Med. News, 1896, lxviii., 339. Discussion by Drs. Mayer, Coakley, Phillips, and Harris.

FEBRUARY 27TH, 1895.—W. Freudenthal, M. D., exhibited a case of angioma of the nasal septum on which he had operated. Discussion by Drs. Delavan, Wright, and Nichols.

W. F. Chappell, M. D., exhibited a case of multiple papillomata of the larynx. Discussion by Drs. Gleitsmann and Phillips.

W. F. Chappell, M. D. Paper: "Treatment of laryngeal tuberculosis by the application and submucous injection of beechwood creosote, with description of an automatic laryngeal syringe." Published: N. Y. Med. Journ., 1895, lxi., 389. Discussion by Drs. Robinson, Smith, Knight, Simpson, Myles, Wright, and Delavan.

MARCH 27TH, 1895.—J. S. Waterman presented a case for diagnosis. Discussion by Drs. Wright, Goodwillie, Quinlan, Delavan, Simpson, and Knapp. (Probably aneurism in tonsillar space.)

D. B. Delavan, M. D. Paper: "Three cases of cystic polypi of the nasal cavity." Not published. Discussion by Drs. Simpson and Myles.

APRIL 24TH, 1895.—F. E. Hopkins, M. D., presented a case in which he had successfully inserted a platinum nasal bridge. Discussion by Drs. Wright and Quinlan.

J. Wright, M. D. Paper: "A few remarks on some cases of mycosis of the nose and throat." Published: N. Y. Med. Jour., 1895, lxii., 1. Discussion by Drs. Delavan, Knapp, Myles, Berens, Gleitsmann, Newcomb, and Knight.

MAY 22d, 1895.—T. J. Harris, M. D., presented a case of sarcoma of the nose cured by operation; also a case of tubercular laryngitis cured by local applications of ichthyol; also a case of adenoma of the nose. Discussion by Drs. Wright, Gleitsmann, Chappell, Berens, Robinson, and Delavan.

J. E. H. Nichols, M. D., exhibited a patient in whom he had successfully inserted an artificial nasal bridge. Discussion by Dr. Hopkins.

W. F. Chappell, M. D. Paper: "Xerostoma or dry mouth." Published: N. Y. Med. Jour., 1896, lxiii., 277.

Drs. Robinson and Richardson each reported a similar case. Discussion by Drs. Richardson, Robinson, and Rice.

C. C. Rice, M. D. Paper: "The importance of administering iodide of potash in cases of laryngeal disease of doubtful diagnosis." Published: Med. Rec., 1895, xlviii, 1. Discussion by Drs. Delavan, Robinson, and Myles.

OCTOBER 23D, 1895.—Address by the chairman, E. Mayer, M. D. Paper: "Deviations of the cartilaginous nasal septum: its cure." Published: N. Y. Med. Jour., 1895, lxii., 748. Discussion by Drs. Delavan,

Asch, Wright, Berens, Simpson, Phillips, and Myles.

NOVEMBER 27TH, 1895.—D. L. Hubbard, M. D., exhibited a patient who was much improved by creosote injections for laryngeal tuberculosis. W. F. Chappell, M. D., presented a similar case who was also much improved by creosote injections. Discussion by Drs. Delevan, Hance, and Myles.

R. Frothingham, M. D., presented a tumor of the larynx for diagnosis. Discussion by Drs. Wright, Simpson, Lincoln, and Mayer.

C. Wagner, M. D. Paper: "Thyrotomy: With report of a series of cases operated upon during the past twenty years." Published Med. Rec., 1896, xlix., 1. Discussion by Drs. Swain, of New Haven, Mayer, and Myles.

DECEMBER 18TH, 1895.—H. B. Douglass, M. D., exhibited a probe-pointed tonsillar knife: also a self-retaining nasal speculum.

L. L. Mial, M. D., exhibited a new pattern nasal saw.

W. F. Chappell, M. D. Paper: "A case of acromegaly with pharyngeal and laryngeal symptoms." Published: Amer. Med. Bul., 1896, ix., 85. Discussion by Dr. J. A. Booth.

G. E. Brewer, M. D., reported a case of total pharyngeal hemorrhage. Discussion by Drs. Bucklin, Brewer, Gleitsmann, and Myles.

J. Wright, M. D., referred to the case of aneurism of the tonsil reported at a previous meeting.

R. C. Myles, M. D., exhibited a case of empyema and polypoid degeneration of the frontal sinuses, cured by double external operation and packing. Dr. Myles also presented a case of central cleft of the soft palate which he had much improved by operation.

L. L. Mial, M. D., presented seven cases of goitre in one family.

J. E. Newcomb, M. D., presented a case of multiple syphilitic lesions.

F. J. Quinlan, M. D., presented a case for diagnosis. (Thought to be an osteoma involving the nasal bones.)

D. B. Delavan, M. D., reported a case in which a large laryngeal papilloma disappeared under the use of alcohol as a spray.

D. L. Hubbard, M. D., reported a case of abscess of the nasal septum. Discussed by Drs. Gleitsmann, Wright, and Rice.

T. P. BERENS, M. D.,
Secretary.

SECTION ON OBSTETRICS AND GYNECOLOGY.

JANUARY, 1895.—H. J. Garrigues, M. D., presented a case on whom sympheseotomy had been performed. He also presented the uterus and appendages of the same case; also another uterus removed by vagina.

C. T. Jewett, M. D., reported a case of inversion of uterus.

H. L. Collyer, M. D. Paper: "Symphyseotomy in a tenement house." Published: Codex Medicus, 1896, ii., 194. Discussed by Drs. Garrigues, Jewett, Harris, Marx, and Vineberg.

R. L. Dickinson, M. D. Paper: "Laceration of the sphincter ani during labor." Published: Am. Gynecol. and Obstet. Jour., 1895, vi., 536. Discussed by Drs. Murray and Jarman.

FEBRUARY 28TH, 1895.—Dr. Tucker presented a modified Tarnier basioprobe.

Dr. A. M. Lesser presented a series of uterine dilators.

Dr. A. F. Currier reported a case of puerpural septicæmia, laparotomy, and recovery.

Dr. A. E. Gallant reported a case of miscarriage with some unusual complications.

Dr. A. H. Goelet showed a modified Cleveland ligature carrier, also a modified Bozeman-Fritch catheter.

Dr. C. P. Noble, of Philadelphia. Paper: "Cœliotomy for puerperal septicæmia, and for puerperal inflammatory conditions." Published: Amer. Jour. Obstet., 1895, xxxi., 801. Discussion by Drs. Boldt, Lusk, Polk, Garrigues, Porter, Waldo, Ford (Philadelphia), Wells, Tucker, Collyer, and Coe.

MARCH 28TH, 1895.—Dr. S. Marx showed tubes, ovaries, and uterus from a patient who had died of septic infection following criminal abortion.

Dr. Garceau, of Boston, showed new instruments for performing vaginal hysterectomy.

Dr. Stuart showed a child with all extremities in the condition of varus and other abnormalities.

A. H. Goelet, M. D. Paper: "Periodic induced currents in gynecology." Published: Amer. Med. Surg. Bul., 1895, viii., 465.

Dr. E. W. Cushing, of Boston. Paper: "Evolution of hysterectomy in America." Published: Internat. Jour. Surg., 1896, ix., 121. Discussion by Drs. Stimson, Baldy (Philadelphia), Polk, Krug, Ricketts (Cincinnati), Goelet, and Cushing.

APRIL 25TH, 1895.—Dr. C. E. Nammack reported a case of spontaneous hæmorrhage from umbilical cord.

Dr. S. Marx showed a uterus at term with fœtus in situ, removed from a woman who died during labor.

Dr. S. Tynberg, by request, showed photographs of twins joined at sacrum.

Dr. P. F. Mundé showed a uterus removed for fibroma: also a fibroid of cervix removed by the vagina.

G. Peckham-Murray, M. D. Paper: "Twin pregnancy, with blighted fœtus." Illustrated by a case. Published: Med. Rec., 1895, xlvii., 711. Discussion by Drs. Coe and Tucker.

E. P. Davis, M. D., of Philadelphia. Paper: "Cæsarian section versus symphyseotomy, with report of case." Published: Med. Rec., 1895, xlvii., 641. Discussion by Drs. Edgar, Lusk, Jewett, Garrigues, Ayers, Coe, and Jarman.

MAY 23D, 1895.—Dr. C. E. Nammack reported a case of cephalopagus craniodidymus.

Dr. H. N. Vineberg presented two patients, with their histories, upon whom he had performed vaginal fixation of the uterus.

Dr. H. C. Coe showed a specimen of ruptured ectopic gestation.

Dr. F. Foerster showed a specimen of a large fibroid pregnant uterus; a fibroma with pus tube; also a 20 pounds fibroma removed per abdomen.

J. Rosenberg, M. D. Paper: "Pregnancy and labor complicated by cardiac disease." Published: N. Y. Med. Jour., 1896., lxiii., 77. Discussion by Drs. Grandin, Baruch, Murray, Marx, and Rosenberg.

J. D. Bissell, M. D. Paper: "Contribution to the study of the nature, cause, and treatment of suspended animation in the new born." Published: Med. Rec., 1895., xlviii., 763. Discussion by Drs. Dew, Harrison, Harris, Grandin, Coe, and Jarman.

OCTOBER 24TH, 1895.—Dr. H. G. Garrigues showed a set of Jacob's instruments for performing vaginal hysterectomy.

Dr. L. F. Bishop reported a case of entanglements of the umbilical cord.

Dr. Pollak reported a case of cystoma of the ovary.

Dr. S. Marx reported a case of ruptured uterus during

labor, also a rupture of vagina and lower uterine segment, both cases seen by him recently.

J. B. Mattison, M. D. Paper: "Morphinism in Woman." Published: Atlantic Med. Weekly, 1896, v., 389. Discussion by Drs. Vineberg, Collyer, and MacLean.

E. H. Grandin, M. D. Paper: "Notes on ectopic gestation." Published: Amer. Med. Surg. Bul., 1895, viii., 1354. Discussion by Drs. MacLean, Brown, Ayers, Tull, Collyer, Jarman, Murray, and Grandin.

NOVEMBER 28TH, 1895.—H. N. Vineberg, M. D. Paper: "Lactation atrophy of the uterus." Published: Amer. Med. Surg., 1896, cxii., 48. Discussion by Drs. Waldo, Coe, and Vineberg.

E. Cushier, M. D. Paper: "Venous thrombosis as a cause of sudden death in the puerperium." Published: Amer. Med. Surg. Bul., 1895, viii., 1519. Discussion by Drs. Vineberg, Waldo, Coe, Jarman, and Cushier.

DECEMBER. Section in charge of a general meeting of the Academy.

GEORGE W. JARMAN,
Secretary.

SECTION ON PEDIATRICS.

JANUARY 10TH, 1895.—Emily Lewi, M. D., and H. Koplik, M. D. Paper: "The milk supply of New York and its availability for infant feeding." The paper was read by Dr. Koplik. Published: Archiv. Pediat., 1895, xii., 122. Discussion by Drs. A. Seibert, R. G. Freeman, E. F. Brush, J. L. Smith, W. L. Carr, and E. Lewi.

FEBRUARY 14TH, 1895.—H. D. Chapin, M. D. Presentation of a boy 16 years old with transposition of the viscera.

H. D. Chapin, M. D. Paper: "The effect of the primary schools on the health of children."

Chas. C. Wehrum, Esq. Paper: "How can the sanitary condition of the public schools be improved?"

M. Morris, M. D. Paper: "Infectious diseases in schools." Published: Sanitarian, 1895, xxxiv., 497.

MARCH 14TH, 1895.—A. Seibert, M. D. Paper: "The use of alcohol in diseases of children." Published: Archiv. Pediat., 1895, xii., 332. Discussion by Drs. Northrup, Kerley, Stowell, and Winters.

W. P. Northrup, M. D., presented a bellows for use in opium narcosis or other case demanding artificial respiration.

APRIL 11TH, 1895.—T. O. Summers, M. D., made a preliminary report on "Experiments with neuclein."

E. B. DENCH, M. D. Paper: "What shall the general practitioner do for acute otitis?" Published: Archiv. Pediat., 1895, xii., 340.

J. E. H. Nichols, M. D. Paper: "Otitis in young children." Not published.

G. Bacon, M. D. Paper: "Cause and prevention of deafness." Published: Amer. Med. Surg. Bul., 1895, viii., 568.

MAY 9TH, 1895.—C. E. Nammack, M. D. Presentation of a negro infant with marked bone deformities.

H. L. Coit, M. D. Paper: "A board of health bulletin on prevention of disease among infants, with presentation of a new pasteurizer."

W. L. Stowell, M. D. "Exhibition of a new centrifugal for determining percentage of fat in milk, of corpuscles in blood, and of precipitate in urine, etc."

S. H. Dessau, M. D. Paper: "What is croup?" Published: Archiv. Pediat., 1895, xii., 421.

F. M. Warner, M. D. Paper: "Report of intubation cases." Published: Med. Rec., 1895, xlviii., 294.

J. H. Fruitnight, M. D. Paper: "The treatment of croup by calomel sublimation." Published: Archiv. Pediat., 1895, xii., 429.

OCTOBER 10TH, 1895.—W. L. Stowell, M. D. Presentation of organs from a case of acute miliary tuberculosis that had shown plasmodium malariæ a short time before.

J. B. Mattison, M. D., of Brooklyn. Paper: "Morphinism in children." Published: Atlantic Med. Weekly, 1896, v., 165.

I. Van Gieson, M. D. Paper: "Infections of the new-born." Not published.

NOVEMBER 14TH, 1895.—J. L Smith, M. D., presentation of history of patient nursed and fed partly on milk. It died of tuberculosis.

R. G. Freeman, M. D., described and exhibited the organs of the same case, showing infection to have originated in the intestine.

Sarah Welt, M. D., presentation of patient with hemiatrophy of muscles and subluxation of hip.

Order. Typhoid fever. The Stamford epidemic of 1895.

F. Schavoir, M. D., Health Officer of Stamford. Paper: "Origin and course." Published: Med. Rec., 1895, xlviii., 803.

W. P. Northrup, M. D. Paper: "Cases occurring in children." Published: Archiv. Pediat., 1896, xiii., 11.

S. Pierson, M. D., of Stamford. Paper: "Clinical aspects of the epidemic." Published: Med. Rec., 1895, xlviii., 804. Discussion by Prof. Herbert E. Smith of Yale, and Drs. E. G. Janeway, A. D. Smith, George L. Peabody, R. C. Newton, and W. M. Brien.

DECEMBER 12TH, 1895.—J. Henry Fruitnight, M. D., presentation of a cretin improving under thyroid extract treatment.

L. W. Hubbard, M. D. Paper: "Early symptoms of joint disease." Published: Amer. Med. Surg. Bul., 1896, ix., 12.

V. P. Gibney, M. D. Paper: "Relaxed ligaments, as in weak knees, weak ankles," etc. Many illustrative cases were shown.

WILLIAM L. STOWELL, M. D.,
Secretary.

SECTION ON NEUROLOGY.

MARCH 8TH, 1895.—E. H. Grandin, M. D. Paper: "Notes on reflex genital neuroses in the female." Published: Am. Med. Surg. Bul., 1895, viii., 399.

P. Bailey, M. D. Paper: "Valvular disease of the heart in tabes." Published: Jour. Nerv. and Ment. Dis., 1895, xxii., 290.

MAY 24TH, 1895.—A. Boyer, M. D. Paper: "The relative importance of labyrinthine and ocular defects in the etiology of vertigo." Published: Annals. d'Oculistique, 1895, cxiv., 348.

L. F. Bishop, M. D. Paper: "The differential diagnosis between localized neuritis, rheumatism, and such other conditions as give similar symptoms." Published: J. Nerv. and Ment. Dis., 1895, xxii., 401.

OCTOBER 11TH, 1895.—J. B. Mattison, M. D. Paper: "A tale of the poppy and its moral." Published: Atlantic Med. Weekly, 1896, v. 228.

DECEMBER 13TH, 1895.—H. S. Stark, M. D. Paper: "Psychical infection: Remarks on the probability of a mental contagium." Published: Med. Rec., 1896, xlix., 554.

A. W. Ferris, M. D. Paper: "Insane patients in private practice: Exhibition of methods of restraint used." Published: Am. Med. Surg. Bul., 1895, ix., 15.

PEARCE BAILEY,
Secretary.

SECTION ON PUBLIC HEALTH, LEGAL MEDICINE, AND HYGIENE.

APRIL 17TH, 1895.—The bill before the legislature "with reference to the coroners of this city," was discussed by Drs. Stephen Smith, Chapin, and others.

A committee to consider this subject was appointed by the chairman, consisting of Drs. Stephen Smith, Henry W. Chapin, J. W. Brannan, F. H. Dillingham, and Robert C. Davis.

NOVEMBER 8TH, 1896.—Discussion on "The relation of bovine to human tuberculosis."

Prof. H. D. Gill. Paper: "Tuberculosis as viewed by a veterinarian." Paper not published.

Prof. L. Pearson. Paper: "The work of foreign veterinarians in tuberculosis." Paper not published.

Prof. F. H. Osgood. Paper: "The control of tuberculosis in Massachusetts." Paper not published.

Prof. J. Law. Paper: "The best methods for the eradication of tuberculosis." Paper not published.

Prof. H. C. Ernst. Paper: "The transmission of tubercle bacilli and tuberculous products in milk." Paper not published. Discussion by Drs. G. B. Fowler, W. H. Thomson, and J. D. Bryant.

ROBERT C. DAVIS,
Secretary.

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